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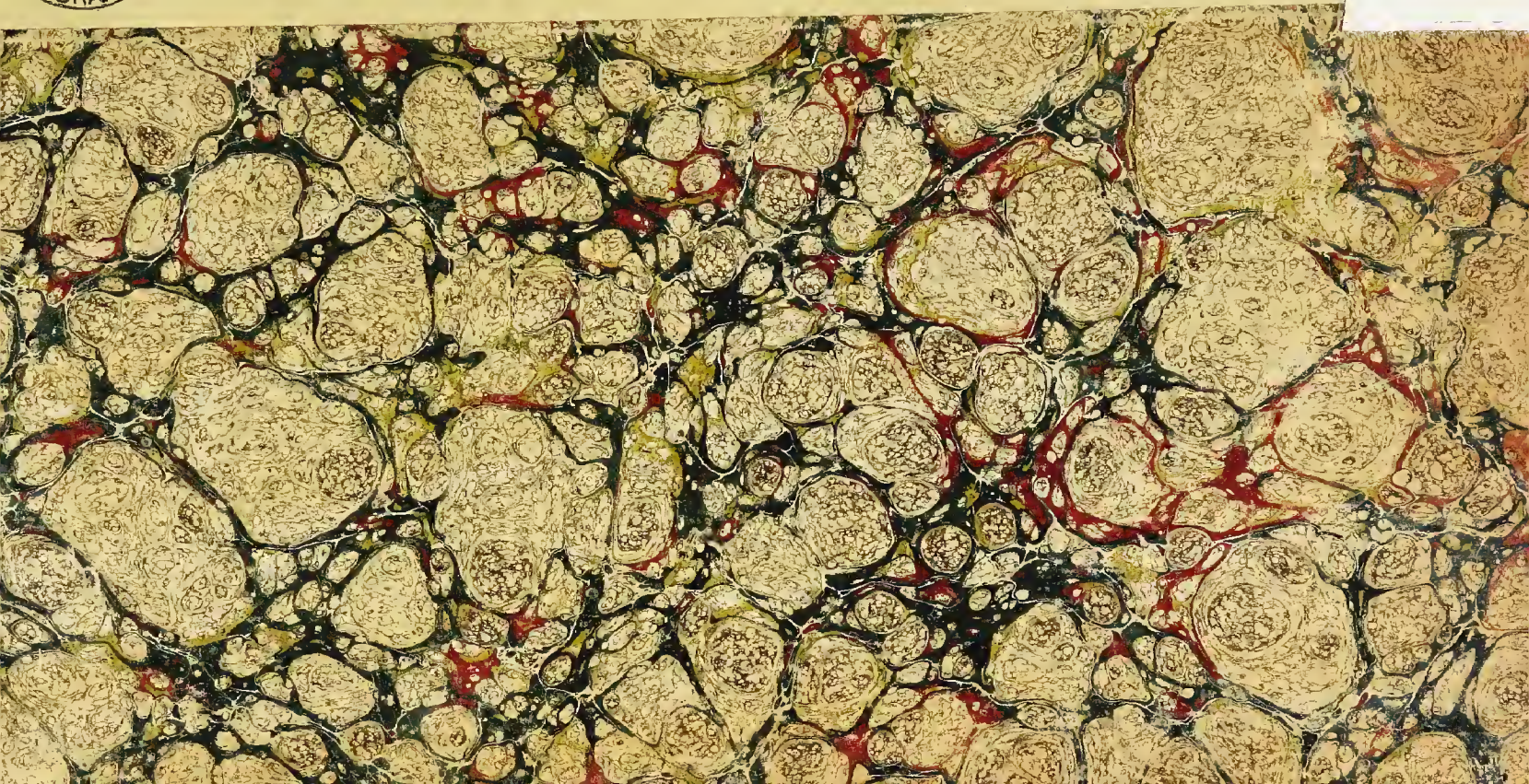

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The Measurement of Attention

BY

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PART I

A CRITICAL STUDY OF PREVIOUS VIEWS AND METHODS

As soon as the experimental method was carried into psychology and applied to mental phenomena, the fundamental importance of attention, which until then had not been generally recognized,[2] became convincingly manifest. There is, indeed, no field of psychological experimentation in which attention does not play a determining part, or, in Ribot's words, "attention is, in fact, the fundamental psychical condi-

[1] From the Psychological Laboratory of Cornell University.

[2] W. James: Princ. of Psychol. I, 1890, 402; Ebbinghaus: Grundz. d. Psychol., 2d ed., 1905, 610 f.; Titchener: Lect. on Feeling and Attention, 1908, 172-174.

tion of almost all psychometrical researches."[1] Results are widely different, according as they are or are not obtained under maximal attention. Hence the precise distinction between various degrees of attention becomes a matter of practical necessity. As such it has made itself felt in two ways. On the one hand, it is often desirable to know how certain results are influenced by the degree of attention, and the experimenter has consequently to use some means of inducing variations of attention, though he need not make any particular effort to measure them accurately. For such purposes two general methods have been mainly employed, the one consisting in distracting the observer from a certain task to which his whole attention is required, and the other demanding that he perform simultaneously two different mental or motor operations. On the other hand, certain investigations have been undertaken with the express purpose of discovering reliable methods and standards for the measurement of all possible degrees from maximal to minimal attention.

Our study of previous views and methods regarding the measurement of attention will naturally divide into three parts. First we shall take up the theoretical discussions concerning the nature of degrees of attention. In the second place, we shall review the general experimental methods usually employed for inducing different degrees of attention without attempt at their accurate measurement. Finally, we shall have to study the special methods and standards proposed or actually used for measuring attention. This, in general, is the programme which we have laid out for ourselves in Part I of the present study. In the second Part we shall submit experimental evidence for the possibility of an adequate measurement of attention in terms of clearness values.

Among the German psychologists, probably the first systematic reference to attention was made by Leibniz[2] in his distinction between perception and apperception. The difference between these two states of consciousness, as Wundt[3] has pointed out, was simply a matter of degrees of clearness. Wundt in accepting this important distinction seems almost surprised to find "how correctly Leibniz had already observed that there were no sudden or abrupt changes but rather gradual transitions between the different clearness degrees of the ideas."[4] Such a view implies, on the one hand, that there can be no absolute limit separating the low degrees of clearness included

[1] Ribot: The Psychology of Attention, Transl., 1896, 67.
[2] The Philosophical Works of Leibnitz. Tr. by G. M. Duncan. The Monadology, paragraphs 19-25, esp. 24.
[3] Wundt: Vorlesungen, 2d ed., 1892, 262.
[4] *Op. cit.*, 262.

in perception from the higher degrees of apperception, and, on the other hand, that there must exist an indefinitely large number of such degrees. A somewhat later and more explicit emphasis upon clearness as an essential factor in attention is found in Wolff's writings. He gives, for example, the following definition: "facultas efficiendi, ut in perceptione composita partialis una majorem claritatem ceteris habeat, dicitur Attentio." As an illustration he mentions the case of a man listening to a conversation so attentively that he entirely forgets his visual surroundings and does not notice the temperature of the room until his attention is called to either the one or the other, when he also loses the thread of the conversation. Many other passages of a similar character could be quoted from Wolff.[1] However, the significant fact for us is that, as Wundt says, "since Leibniz the distinction between clear and obscure ideas has remained an almost uncontroverted possession of psychology."[2] Certainly it was quite familiar to the German psychologists of the 18th century, as a few references from Dessoir will suffice to show.[3] Sulzer (1720-1779), for example, speaks of attention as "das Licht der Seele" and points out "dass durch vermehrte Aufmerksamkeit ein blosses Bild der Phantasie das Leben eines wirklichen Gegenstandes bekommeu und hingegen der wirkliche Eindruck durch den Mangel der Aufmerksamkeit zu einer sehr schwachen Vorstellung werden kann." Hentsch (1723-1764) says: "diese Bemühung, auf einen Teil einer vorkommenden Sache mehr Acht zu haben, als auf die übrigen, nennen wir die Aufmerksamkeit"; while von Irwing (1728-1801) thinks that "die Klarheit und Lebhaftigkeit aber allein ist nicht hinlänglich, um eine Idee bis zur Apperception zu bringen."

The psychological writings of a later period, especially at the beginning of the 19th century, were too seriously influenced by the philosophical interests and theories of their authors to give unbiassed consideration to our topic. At the same time, we have already arrived at the "turning-point of modern psychology,"[4] for in Herbart's system the problem of apperception and attention begins to assume fundamental importance. His discussion of the limen of consciousness and of the rise and fall of competing ideas can easily be interpreted as referring to the increase and decrease of the clearness of ideas. This is particularly true of his *Psychologische Untersuchung über die Stärke einer gegebenen Vorstellung als Function ihrer*

[1] Psychologia empirica, 1732, par. 237; *cf.* par. 235-238, and 592; Psychologia rationalis, 1734, par. 357, 358, 360, 367, 369, 372.
[2] Wundt: Vorlesungen, 2d ed., 262.
[3] Dessoir: Gesch. d. neueren deutschen Psychologie, 1894, 235-237.
[4] Titchener: Exper. Psych., Vol. I, Part II, 186.

Dauer betrachtet.[1] While Herbart was the first to apply mathematical formulas to mental phenomena, he nevertheless thought it impossible to perform experiments upon human beings, and he thus implicitly denied that psychology could become an exact science. Lotze, on the other hand, with his expert medical knowledge and his deep psychological insight, helped indirectly to affiliate psychology to the other natural sciences. He seems to have been the first to introduce into modern psychological literature the analogy of attention and inattention to the Blickpunkt and Blickfeld of vision. He says: "the mind is not so constituted as to experience all its (simultaneous) contents with equal clearness and attention. It is rather to be compared to the retina of the eye with its singularly sensitive fovea around which a large area of symmetrically decreasing sensitivity is centred. Just as in the eye every peripheral point, in spite of its indistinctness, has nevertheless its definite position with regard to the more distinct centre, so in the course of ideas in the mind the more suppressed excitations centre around the clear focus of attention and, without disturbing the focal content, contribute to the greater richness of consciousness and to its peculiar moods or states of illumination."[2] Lotze also speaks of a "Steigerung der Aufmerksamkeit" which manifests itself in peculiar changes of the "Helligkeit gewisser Sinneseindrücke,"[3] a phrase which evidently refers to the clearness of the sensation, since the intensity of the stimulus is assumed to be constant.

With the beginnings of experimental psychology in the middle of the 19th century, a new emphasis was laid upon attention. Many years before Wundt had started his investigations with the complication pendulum and the tachistoscope, Fechner had been led by his quantitative study of the psychophysical relation to distinguish sharply the limen of sensible intensity from the limen as influenced by the degree of attention. As early as 1860 he wrote, for instance: "We can think or have ideas, weak as to their content, but strong as to their intensity. . . . Hence the intensity of the idea and the strength with which we think or perceive it must be in some way distinguishable from each other".[4] In 1877 he was still more explicit on the same point: "If we perceive a sensory phenomenon or represent it to ourselves in the form of an idea, the intensity of our conscious activity is then determined on the one hand by the degree of attention with which we perceive

[1] Herbart's Sämmtliche Werke, ed. by Karl Kehrbach, 1888, Vol. III, 121-145, esp. 123. *Cf.* Lehrbuch z. Psych., Werke VI, 140 ff.
[2] Lotze: Medicinische Psychologie 1852, 505.
[3] *Op cit.*, 510.
[4] Fechner: El. d. Psychophysik, 2d ed., Vol. II, 453.

the sensory or the memory image, and on the other hand by the vividness or intensity which pertains to the phenomenon itself. . . . In such cases we can quite well distinguish how much is due to the degree of attention and how much intensity belongs to the phenomenon as such".[1] A very similar passage occurs in the *Revision:* "If I look at a more or less bright surface or listen to a more or less loud sound, then I can very well distinguish the degree of attention with which I perceive it from the degree of brightness or of intensity of the sound which I experience".[2] He even goes further and adds: "the strength of the attention may decrease while the intensity of the phenomenon, as, *e. g.*, the brightness or sound sensation, may increase, and conversely".[3] Now what else could such repeated emphasis upon the distinction between degrees of psychophysical intensity and degrees of attention indicate, if Fechner did not wish to differentiate as sharply as possible between intensity and clearness? Although it is true that he himself nowhere uses the term clearness in connection with degrees of attention, yet it is equally certain that there is no other sensational attribute than that of clearness which could be thus fundamentally opposed to the attribute of intensity. It is perhaps due to Fechner's avoidance of the term clearness that he did not state more explicitly the inevitable conclusion implied in these passages, that the degree or strength of the attention given to a certain mental process is equivalent to the degree of clearness which this process attains as compared with the other mental processes of the moment.

In this respect Stumpf seems to go beyond Fechner, for in the second volume of the *Tonpsychologie* there is a passage which indicates that for Stumpf clearness is not only essential to attention, but also that it increases as attention itself increases. He says: "the effect of attention in analysing is capable of an almost unlimited increase. An analysed clang and a particular tone in it may in all inner characteristics and external relations, in all respects and in all directions become more and more distinct".[4] Here the terms "Steigerung" of attention and "immer deutlicher werden" are plainly synonymous. With regard to the psychophysical basis of attention Stumpf accepts and defends[5] Fechner's analogy of the wave form against G. E. Müller's criticism,[6] and quotes Exner[7] in his support.

[1] In Sachen d. Psychophysik, 1877, 85 f.
[2] Revision d. Hauptpunkte d. Psychoph., 1882, 270.
[3] *Op. cit.*, 270.
[5] Tonpsych. I, 1883, 70, footnote.
[4] Stumpf: Tonpsychologie II, 1890, 288.
[6] Müller: Grundl. d. Psychophysik, 1878, 359.
[7] Pflüger's Archiv XI, 1875, 429.

In Lipps' discussion of attention, its relation to clearness is made still more explicit. The function of attention is to "increase that energy of the excitations with which they are to gain independent consciousness",[1] and Lipps sharply distinguishes this increased mental energy from the stimulus intensity. In another passage he gives an admirable description or definition of clearness: "it is indeed possible for ideas to break their way into consciousness with greater or less strength, to remain there a longer or shorter time, and accordingly to make a more or less energetic impression, to enter into more or less close relations with other mental contents, to be more or less appropriated by the total conscious situation and thus to occupy in it a more or less dominating position".[2] The same thought occurs twenty years later in a more concise form in his *Leitfaden*, where he says, for example: "the real psychical fact which lies at the basis of attention may be defined as a more or less strong psychical effectiveness of the processes upon which the attention is directed",[3] or: "the clearness degree of a sensation or idea would then be only another name for its degree of effectiveness in the mental life".[4] "Instead of psychical effectiveness", Lipps writes elsewhere, "we may just as well say: psychical force. We speak of force when we refer to some effect or work done. And we measure the magnitude of the force by its effects. Thus the amount of attention directed to an ideated object is nothing but the psychical force of the idea of this object".[5] This means in other words that, in order to measure the attention given to a certain mental process, we have to consider its "psychical effectiveness", or better, the degree of clearness with which it appears in consciousness.

Külpe agrees with Fechner, Stumpf, and Lipps, when he writes: "It is a matter of familiar experience that the vividness or degree of attention is itself capable of quantitative gradation, and that the differences may be distinguished from differences in the intensity of sensations."[6] However, he is very sceptical as to the fruitfulness of the experimental methods employed in securing and measuring degrees of attention, and closes his discussion by saying: "The discovery of a reliable measure of the attention would appear to be one of the most important problems that await solution by the experimental psychology of the future".[7] Ten years later he prac-

[1] Lipps: Grundtatsachen d. Seelenlebens, 1883, 134.
[2] *Op. cit.*, 42 f.
[3] Leitfaden: 1903, 34.
[4] Leitf: 38.
[5] Leitfaden: 34.
[6] Külpe: Outl. of Psych., 1895, 441.
[7] Külpe: Outl., 429.

tically repeats this statement in the following passage: "It is clear that, if we are to induce a determinate degree of consciousness, we must know, first, the mode of distribution of the total energy of attention over the separate conscious contents included within its range, and, secondly, the distractive value of the process which we have selected for our purpose. Now no general rule can be laid down under either heading; the results that have been obtained hold only in particular cases; so that, from this point of view also, the investigation of attention is peculiarly difficult. So much we may say, and no more: that the distribution of attention brings with it a reduction, and concentration of attention an enhancement, of the degree of consciousness for a determinate content. Psychologists have not yet attained to any quantitative formulation of the changes".[1]

A similar complaint was made in the same year, as well as two years later, by Ebbinghaus: "A desirable aim for the study of the distraction phenomena would be the securing of exact and more or less generally valid figures. . . . However, it is clear that the prospects for accomplishing this aim under the described complication of things are not very bright".[2]

It is difficult to state correctly Wundt's attitude toward the problem of measurement of attention, since he makes no explicit references to it. As was said above, Wundt accepts Leibniz' distinction between clear and obscure ideas, between apperception and perception,[3] and he also seems to agree with Leibniz as to the possibly endless number of intermediate degrees between these two extremes. Since they are always accompanied by feelings of strain, the intensity of the feelings may, according to Wundt's earlier writings, serve as an indication of the probable degree of apperception or clearness. There seems here to be an ambiguity in Wundt's terminology, in as far as he uses synonymously *Spannungsgefühl* and *Spannungsempfindung*. But since strain, according to him, constitutes the main element of the *Thätigkeitsgefühl*, since in his later pluralistic theory of the affections it is one of the six dimensions of feeling, and finally, since he himself objects very strongly to Münsterberg's wide-reaching use of the muscular and other bodily strain sensations, it seems probable that even in his earlier writings he intended to classify strain among the feelings. It is then not surprising that in other psychological treatises, referring to Wundt's earlier writings, the terms sensation and feeling of strain are likewise confused. Mentz, for

[1] The Problem of Attention, Monist XIII, 45.
[2] Ebbinghaus: Grundz. d. Psych., 2d ed., 1905, 623 f.
[3] Wundt: Vorlesungen, 2d ed., 262.

example, uses as a basis for a number of experiments the following passage in Wundt: "Those strain sensations which, with the same attention, accompany the external volitional act as well as the direction of the will to the particular sense departments, form a complex of qualitatively related sensations which is characterized by a constancy attained by no other conscious content. These sensations are always present, —which cannot be said of any other sensory process; and through their various degrees of intensity they offer at the same time a direct measure of the energy of any given volitional activity".[1] Mentz, accordingly, gives his observers the following instruction: "The reactor is required voluntarily to increase his attention in equal degrees, as far as this is possible, and to report afterwards what particular degree he has given", and goes on to say: "Wundt has already remarked that the strength of attention may be measured by the intensity of the strain sensations".[2] Again, Wundt says, after speaking of the relation of association to active and passive apperception, that "in both cases the apperception, as a process which differs from the mere course of ideas, comes to consciousness partly by means of the accompanying feelings and partly by means of the accompanying strain sensations whose intensity increases with the degree of attention".[3] This leads Pillsbury to make the statement that, according to Wundt, "the feeling of activity and the sensations of strain are most intensive when the ideas are clearest. Increase in the clearness of ideas and in intensity of strain sensations, etc., always go on side by side".[4] Hence Mentz was justified in his interpretation of Wundt, and on this basis his observers distinguished, according to his tables, in the one case four and in the other case six degrees of voluntary attention. Unfortunately, Mentz does not give any further description of these degrees and adds only incidentally the remark "that just this increase (probably of strain) with the higher degrees was rather difficult to accomplish".[5] Wundt himself, in the 5th edition of the *Physiologische Psychologie*, has made a sharp distinction between the feelings and the sensations of strain. He now connects the feelings of strain with the intensity of the sensory processes attended to, because they inform us of our correct bodily accommodation to the external stimulus and thus indirectly help us to bring it to its full intensity. He says: "Furthermore we notice that the degree of the feelings of strain

[1] System d. Philos., 1889, 100.
[2] Mentz: Phil. Stud., XI, 1895, 574.
[3] Grundz. d. physiol. Psych., II, 4th ed., 1893, 279.
[4] Pillsbury: *Amer. Jour. Psych.*, VIII, 1896, 279.
[5] Mentz: *op. cit.*, 576.

keeps step with the intensity (*Stärke*) of the impressions. Upon the exactness of the occurring accommodation depends what is called the keenness (*Schärfe*) of attention".[1] By this keenness he means simply the accuracy of the sense organ involved. After all, then, we are still left in the dark as to Wundt's attitude toward the problem of measuring attention. It seems hardly possible that he would deny the existence of such a problem, although his refusal to make any direct reference to it or to the large literature in question might perhaps be interpreted in this way. However that may be, this much is certain: Wundt at least admits a large number of degrees of attention and makes them equivalent to degrees of clearness ranging all the way from the highest level of apperception to the lowest level of perception. On this point he agrees with all the other German authors quoted thus far; in fact, his insistence upon clearness as the essential characteristic of attention has become the central aspect of his whole system of psychology.

In Münsterberg's treatment of attention great stress is laid upon kinæsthetic strain sensations. They are said to arise not only in the muscles involved in the adjustment of the particular sense-organ, but also in the muscles of the head, the neck, and the chest, especially in the muscles involved in respiration. Münsterberg holds that "the greater the demands that are made upon the efficiency of the sense organ, the more intensive will all these strains have to be, for the adaptation (*Einstellung*) takes place the more exactly, the stronger the tension in the antagonistic muscles. . . . The intensity of the strain sensations is therefore generally accepted as a measure of the strength of attention; thus the latter may be increased considerably while the impression itself remains constant".[2] Külpe, however, makes the criticism, which is based upon introspective evidence, that "there is no simple or necessary proportionality between the intensity of strain sensations and the degree of attention".[3] This objection cannot be satisfactorily answered, and therefore Münsterberg's criterion for degrees of attention is psychologically impossible.

The earliest and perhaps the only systematic French treatise on attention is written by Ribot. He also admits the existence of many degrees of attention, and even attempts their graphical representation. He says: "graphically we might represent the totality of its normal and morbid manifestations by a straight line. . . . At the centre, let us put ordinary

[1] Grundz. d. physiol. Psychol., 5th ed., III, 1903, 337.
[2] Münsterberg: article on Aufmerksamkeit in the Real-lexikon der Medic. Propädeutik by Joh. Gad, Vol. I, 1893, col. 532-539.
[3] Külpe: Outlines, 428.

spontaneous attention. Following our imaginary line to the right, in the direction of increasing attention, we find strong spontaneous attention, then preoccupation, then the weak fixed idea. . . . Reverting to our starting point, we now turn to the left, in the direction of decreasing intensity. Here we have voluntary attention, at first in the form of an organized habit, then in its general ordinary form, then vacillating. . . . Between each form and its adjacent ones, there occur shades which I omit to notice".[1] As to the nature of these "shades", Ribot is thus not very explicit; but we can easily infer from the passage quoted as well as from many other statements that he must have had in mind differences in clearness. This is, for instance, evident from the following statement: "I am, accordingly, fully inclined to hold, with Buccola, that the fixed idea is attention at its highest degree—the extreme limit of its power of inhibition".[2]

Much less unanimity of opinion both as to the number and the nature of degrees of attention exists among the English and American psychologists. Ward, for instance, seems to agree with Ribot and the German school, although he, too, is not very explicit. He distinguishes between "the intensity or concentration of attention and its diffusion or the extent of the field of consciousness".[3] Still, he makes "loss of intensity" correspond to loss of "distinctness" in the following passage: "if, then, attention be distributed over too wide a field, there is a corresponding loss of intensity, and so of distinctness". He also seems to imply a scale of gradual changes "between the two zeros of complete indifference and complete absorption". Practically at the same time Sully published his first works on psychology. As he was one of the first English systematic writers to give attention an independent place, it is interesting to find him also in agreement with the Germans so far as degree of attention is concerned. He says, for instance: "the field of consciousness, however, is wider than that of attention. Consciousness admits of many degrees of distinctness. . . . To attend is to intensify consciousness by concentrating or narrowing it on some definite and restricted area. It is to force the mind or consciousness in a particular direction so as to make the objects as distinct as possible".[4] However, since he thinks it best "to reserve the term attention for the more palpable exertions of mental activity in definite directions",[5] his terminology is different from that of previously

[1] Ribot: The Psychology of Attention, 104-105.
[2] *Op. cit.*, 86. Other evidences are quoted in Titchener's Lectures,
[3] Ward: art. on Psychology in the Encycl. Brit., 1886, 70. [184.
[4] Sully: Outlines of Psychology, 2d ed., 1885, 73.
[5] *Op. cit.*, 75, footnote.

mentioned writers. Especially the terms activity and energy play an important part in his later treatments of attention, as may be seen from the following passage: "the amount of attention exerted at any time depends on two chief circumstances, (*a*) the quantity of active energy disposable at the time; (*b*) the strength of the stimulus or force which excites the attention or rouses it to action. If there is great active energy a feeble stimulus will suffice to bring about attention. The healthy, vigorous child in the early part of the day has a superabundance of energy which shows itself in attention to small and comparatively uninteresting matters. On the other hand, a tired or weakly child requires a proportionately powerful stimulus".[1] Here the reference to clearness is replaced by a strong emphasis upon the hypothetical mental energy.

Practically the same thing holds of Ladd's discussion of the nature of attentive degrees. He begins by following Wundt, for example when he writes: "that changes in the clearness of perception take place in dependence on the changes in the degree of attention is a matter of the most ordinary experience,"[2] or: "by gradual increase in the intensity of attention (changing the casual glance into a steady look) these objects become apperceived more and more clearly."[3] However, he adds to this plain statement of a psychological fact the physiological hypothesis that "under the influence of attention the cerebrum has become more susceptible for certain impressions, less so for certain others. Stored energy of the nerve cells is being rapidly called forth. Concentrated voluntary attention implies a large amount of work being done in the cerebral hemisphere."[4] Hence he concludes in a somewhat puzzling way that "by the amount and speed expended in attention we measure in a large degree the extent and intensity of consciousness."[5] According to Ladd "primary attention, essentially considered, is the variously related degrees of psychic energy expended upon the different aspects, elements, and objects, in the one field of consciousness."[6] The admission "of an indefinite number of degrees of (conscious) energy" he thinks leads one to ask, for instance, "how much can I feel without losing consciousness?" or "how vivid can my memory-image or picture of imagination become as measuring the utmost capacity for vividness which my conscious mental life can display in this way?"[7] These questions again refer to at-

[1] *Op. cit.*, 79.
[2] Ladd: El. of Physiol. Psych., 1889, 541.
[3] *Op. cit.*, 541.
[4] *Op. cit.*, 543.
[5] *Op. cit.*, 544.
[6] Psych. Descr. and Explan., 1894, 74-75.
[7] *Op. cit.*, 42.

tention as essentially characterized by clearness, and thus we find Ladd's discussion of attention wavering between two different standpoints.

According to Baldwin, the whole area of consciousness may be subdivided into five levels or degrees. The lowest is beyond consciousness; it is the physiological region of the Unconscious. Then follows the Subconscious, next Passive or Diffused Consciousness, then Active Consciousness or Attention, and finally the level of Apperception.[1] In another passage he likens attention to "the line of mental vision," or to "the visual field in which objects are scattered, those being most clearly seen which are in the line of direct vision or centre of the field. Between these limits," he immediately adds, "there are all degrees of distinctness."[2] This last sentence would seem to imply that each of the four or five levels must itself include a large number of lesser differences, although perhaps no hard and fast distinction could be drawn between them. As to the nature of these degrees we are left to infer that they are differences of clearness. Such an implication is for instance made in his discussion of the relation of attention to the intellect, where he says: "in general, it may be said that attention increases the vividness of representative states,"[3] and lower down on the same page: "the capacity to retain mental pictures depends upon the intensity of the original presentation, and the clearness of its relations; and this intensity and clearness are enhanced by the attention." However, in Baldwin's later writings the clearness factor is entirely disregarded, a fact due probably to the dynamogenetic point of view. In this respect he is not followed by J. R. Angell, who otherwise seems to accept Baldwin's view as expressed in his earlier discussion of attention, even reprinting the diagram of the five concentric circles which represent the different levels of consciousness.[4]

The latest systematic treatise on attention, that of Pillsbury, again emphasizes the clearness factor. He admits various degrees of attention, and identifies them with degrees of clearness: "increase in the degree to which an impression is conscious and increase in attention to that impression are synonymous."[5] In his sixth chapter he discusses in some detail "the Methods of Measuring Attention" and makes special reference to the experimental work on this problem, which we, too, shall have to consider later on. For the sake of com-

[1] Baldwin: Senses and Intellect, 1889, 68.
[2] *Op. cit.*, 64.
[3] Baldwin: *op. cit.*, 75.
[4] Angell: Psychology, 1904, 65-67.
[5] Pillsbury: Attention, 1908, 2.

pleteness we should mention in this place Titchener's latest discussion of our topic, but since it also is intimately connected with the experimental work upon attention we reserve it likewise for later treatment.

To sum up our review of the theoretical discussions concerning the nature and number of degrees of attention, we find that most of the writers, as far as they are explicit on this point, agree that there are an indefinitely large number of such degrees, and that the degrees consist in differences of clearness of the conscious processes attended to or distracted from.

We are now ready to pass to the experimental work upon different degrees of attention. Here we have, first of all, to distinguish between the general means of inducing variations of attention on the one hand, and the special methods proposed or used for the sake of standardising or accurately measuring these variations on the other hand. Of course, all the investigations here in question had to employ some means of varying the attention; but some of them did not go beyond this, simply because their main problem lay elsewhere. Nevertheless, in our present discussion of the various means of inducing different degrees of attention, we shall not confine ourselves to this class, but shall include also those of the other class, because this procedure will enable us to reduce unavoidable repetition of references to a minimum. Later on, when we come to discuss the special methods of standardising degrees of attention, the investigations of the first kind will naturally have to be omitted.

The main question for any one who attempts to vary attention is this: How can an observer be induced (or forced against his will) to give less than maximal attention to a prescribed task? The choice actually made among the numerous possibilities that offer themselves has depended partly upon the nature of the given task, and partly upon the main purpose of the particular investigation. Some of the authors have found it necessary to use various means. Nevertheless, in general, all procedures may be divided, as has been said in the introduction, into two great classes, which we may briefly characterise as the "single-task" method and the "double-task" method, sometimes called the method of distraction and the method of simultaneous activities. These two methods are intimately connected with the basal fact of consciousness that at any given moment only a limited number of mental processes may be attended to, that is, may rise to focal clearness, while other simultaneously present processes occupy the relatively obscure background. In the single-task method, as the name implies, either the processes constituting the prescribed task

are to be attended to, while other processes, constituting what is usually called the distraction, are artificially introduced in the background in order to reduce indirectly by their strength and number the attention given to the focal processes of the task; or, more frequently (and especially in the later experiments), the mental processes of the distraction are made focal while the original task is relegated to the background. In the double-task method, on the other hand, two sets of mental processes are introduced which are intended to occupy simultaneously the focus of consciousness, while the backgrouud processes are not taken into account. Külpe was the first to suggest this distinction, when he said: "experience shows that there are but two ways, in the normal waking state, by which this end may be accomplished. We may either distract the attention, or we may divide it".[1] In both cases, the variation of attention is inferred from the change in quality or quantity (or both) of the task performed.[2]

The single-task method is, so far as our knowledge goes, chronologically the older of the two. It seems to have been first employed by Wundt[3] and Obersteiner[4], as early as 1874. Obersteiner, who attempted to measure attention by means of the reaction time, used auditory, cutaneous, and visual distractors. He says: "I placed a musical box which played softly in his vicinity", or "I applied a tolerably strong induction current to the left arm", while in other series the reactor "looked into a kaleidoscope with changing figures".[5] Obersteiner assures us that his observer "always endeavored to abstract from the disturbing ifluences and to concentrate his attention on the reaction".[6] A few years later, Boas, in connection with his work on the difference limen, reported experiments on the determination of the difference limen of brightnesses in which "the attentive listening to a piece of music" was used as a distraction. Here, then, we notice for the first time

[1] Külpe: The Problem of Attention; The Monist, XIII, 1903, 44 f.

[2] Sometimes the term distraction has been applied to one of the two tasks which had to be attended to simultaneously. Or, the double-task method has been called the method of simultaneous activities. This has resulted in a good deal of confusion, since the single-task method likewise involves simultaneous activities, the difference consisting mainly in the direction of the attention. Therefore we have thought it better to employ the new terms "single-task" and "double-task" method as indicating the essential difference between the two, instead of following Külpe and calling them the methods of distracted and distributed attention.

[3] Wundt: Grundz. d. physiol. Psychol., 1st ed., 1874, 745-749.

[4] Obersteiner: Virchow's Arch. f. pathol. Anat. and Physiol., LIX (N. S. IX), 1874, 427-458.

[5] Obersteiner: Brain I, 1879, 447 f.

[6] *Op cit.*, 449.

that the observer did not attend to the original task of brightness discrimination, but to the distraction, so that the former was relegated to the periphery or background of consciousness.[1] Boas' results were known to Stumpf, to whom they perhaps suggested a new method of standardizing the different degrees of attention, which we shall later discuss in detail under the heading: method of graded distractors. The first criticism of sensory distractors, as unable to reduce indirectly the attention given to a prescribed task, was made by Cattell in 1886, in his investigation of the influence of degrees of attention upon reaction-time. He "let three metronomes beat and ring rapidly",[2] and he found that "the attention can be more thoroughly distracted if the brain is busied with some other operation while the reactions are being made. A good way to accomplish this is to let the subject beginning with any number add as rapidly as possible 17 after 17 to it".[3] It remains doubtful even here whether in the few moments preceding and including the reactions the subject's attention was not quickly withdrawn from the addition-task. Münsterberg has used both the single-task[4] and the double-task[5] method, employing as a rule addition as a distractor. In some of Bliss' experiments it was desirable to note the influence of attention upon the task of tapping, and therefore "an effort was made to distract the attention of the person tapping" by weak sounds, but without effect, while on the other hand "the blowing of a loud whistle was followed by a great irregularity", as happened also with "the mental addition of 214 and 23" and the "mental multiplication of 14 by 5".[6] Swift reports that muscular reaction-times were lengthened "while a metronome was ticking one hundred and twenty times each minute".[7] He also made some experiments "to find how the simple 'muscular' and the 'choice' reactions would vary while the reactor's attention was directed to certain kinds of work. Three tasks were given: 1, repeating a poem already committed to memory; 2, reading an English book; and 3, reading Kant's *Kritik der reinen Vernunft*. . . . The instructions were to fix the attention as closely as possible on the work assigned".[8]

In the years from 1895 to 1900 there appeared a relatively large number of experimental articles bearing upon our problem. The single-task method was mainly used in the in-

[1] Boas: Pflüger's Archiv, XXVI, 1881, 496.
[2] Cattell: Mind, XI, 1886, 237.
[3] *Op. cit.*, 238.
[4] Münsterberg: Zeits. f. Psychol., I, 1890, 104.
[5] Münsterberg: Beiträge z. exp. Psychol., IV, 1892, 200.
[6] Bliss: Stud. fr. the Yale Psychol., Lab., I, 1893, 50.
[7] Swift: *Amer. Jour. Psychol.*, V, 1893, 5. [8] *Op. cit.*, 17.

vestigations of Hamlin, Moyer, Birch, and Darlington and Talbot, done under Titchener's direction in the Cornell laboratory. Hamlin,[1] repeating some of Münsterberg's[2] experiments for the purpose of measuring the effect of attention upon sensible intensity, found that addition as the distracting task could not be used effectively together with judging small differences of intensity, brightness, and pressure, because "the subjects usually found that it acted as a spur rather than as a check to the attention. Their attention was wholly absorbed in the addition for a part of the interval, but as a rule the distraction was not continuous, and there came free instants in which the stimulus flashed into consciousness with great clearness and distinctness. Although the subjects were instructed to give their attention to the addition, the fact that they must give some judgment of the stimuli made them instinctively attentive to the stimulus, whenever there was any break in the continuity of the distraction."[3] In Moyer's experiments the distractions to be attended to were, besides the mental addition of three-place figures, "writing the words of a sentence in reverse order, writing the letters of a word backward, translation of simple sentences into a foreign language and writing the words and letters in a reverse order, and discrimination of odors."[4] Birch[5] continued and extended the study of odors as distractors, while Darlington and Talbot used musical "phrases of nine notes each, played upon the piano. Al were written in $\frac{2}{4}$ time and in the key of C. Five different octaves were used, the notes ranging from the c^{-1} to the c^{4}". Important work on attention has also been done by de Sanctis, who in some of his experiments aimed at a measurement of the intensity of concentrated and distributed attention. He required the observer "to occupy himself with a (tapping) movement which had to be made simultaneously with the beats of a metronome and under total concentration of attention. . . . While the observer was thus busy, certain distractions were induced in him, being made progressively stronger and more numerous; at the same time he was required to close an electric current whenever the distraction intruded into the focal consciousness."[7] In order to study the variations of attention under the influence of different conditions, Binet and Henri[8] have used as a distraction two metro-

[1] Hamlin: *Am. Jour. Psychol.*, VIII, 1896, 1-66.
[2] Münsterberg: Psychol. Rev., I, 1894, 39-44.
[3] Hamlin: *op. cit.*, 49.
[4] Moyer: *Amer. Jour. Psychol.*, VIII, 1896, 411.
[5] Birch: *Amer. Jour. Psychol.*, IX, 1897, 45-55.
[6] Darlington and Talbot: *Amer. Jour. Psychol.*, IX, 333.
[7] Sante de Sanctis: Zeits. f. Psychol., XVII, 1898, 209.
[8] Binet et Henri: L'année psychol., III, 1897, 237.

nomes of slighly different rates, "par exemple 50 et 60 à la minute," with the attention on the original task, while Toulouse and Vaschide[1] employed odors for the same purpose.

After 1900 the interest in the problem of measuring the attention seems to have somewhat relaxed. The experimental work was, however, continued by Wirth and Peters. The latter attempted to find a measure of attention in the rise and fall of the stimulus limen, and used the single-task method in order to eliminate the *Einstellung* of the attention to a liminal stimulus. "*Einstellung* to the liminal stimulus," he says, "is lacking in the experiments in which the observer had to concentrate upon one of the following four contents: reading, mental arithmetic, reading Hungarian, pulling the spring in a spring balance. . . . In these experiments not only is the *Einstellung* lacking, but we have also a new phase, the concentration upon a different content from the expected liminal impression."[2] There remains only to be mentioned Ebbinghaus' recommendation of using "geeignete Hauptleistungen und geeignete Störungen"[3] in order to obtain a more exact measurement of the attention.

The double-task method seems to have been much less frequently employed in psychological experiments. Perhaps the first use of it was made by Loeb[4] in 1886. His aim was to find out whether muscular activity could be used as a measure of mental activity. One of his two tasks was to give "the maximum pressure which the flexor muscles of the hand can exercise upon a dynamometer," while the observer was reading or making multiplications of different degrees of difficulty. His work was continued and extended several years later by J. C. Welch, who correlated the muscular strength of the hand with many different mental activities,[5] for instance, visual and auditory perception and the registration of rhythmical pendulum movements, mental adding and multiplication, reading, writing, and repeating poetry. Münsterberg has frequently and for various purposes used adding simultaneously with other kinds of observations, but he has also employed, especially together with large arm-movements,[6] such tasks as reading, counting letters, and fixating dots. Smith investigated "the relation of attention to our power of associating and recollecting objects presented to our consciousness", and

[1] Toulouse et Vaschide: Compt. rend. de soc. de biol., XI, 1899, 965.
[2] W. Peters: Arch. f. d. ges. Psychol., VIII, 1906, 406.
[3] Ebbinghaus: Grundz. d. Psychol., I, 2d ed., 1905, 623.
[4] Loeb: Arch. f. d. ges. Phys., XXXIX, 1886, 592.
[5] Welch: Amer. Jour. of Phys., I, 1898, 283-306.
[6] Münsterberg: Beitr. z. exper. Psychol., IV, 1892, 200.

found[1] among other results that a "distinct but not too complicated device for securing what one may call mental distraction (and what he elsewhere better terms "variation of attention") consisted in requiring the subject, while learning the letters on the card (for the sake of memorizing them) to perform a simple sum in addition, viz., that involved in repeating the sum 2, 4, 6, 8, . . . or rarely, when the former series tended to become too mechanically easy, the series 3, 6, 9, . . . In order that an effective control over the behavior of the subject might be secured, he was required farther to repeat the numbers aloud and to make each step in the addition coincide with the stroke of a metronome beating at the rate of 60-70 strokes per minute."[2] In Drew's experiments the observer had to make reactions "on five and ten-fingered keyboards" together with reciting the "multiplication table up to 12×12, . . . reading text (1) silently, (2) aloud, naming letters (1) in direct order in text, (2) in reverse order, (3) on drum, seen one at a time. . . . Reaction made by finger key and with mouth key; to be made in shortest possible time."[3] As to the purpose of this series of experiments, it is interesting to note that Drew says: "its object was to arrange a series of tasks of increasing degrees of complexity which should make ever greater demands on the mind, until the attention should pass from a fully concentrated to a completely distracted state."[4]

Perhaps the most frequent use of the double-task method has been made by Binet and Henri. For instance, they required an observer to press rhythmically a rubber bulb connected with a Marey-tambour tracing on a kymograph. At the same time he was asked to perform some difficult mental task such as adding, reciting, or reading aloud.[5] In another case, Henri[6] required his observers to read and write simultaneously two different pieces of prose. In this way Binet and Henri tried to find an answer to the question "how can we know whether a person gives much or little attention to a certain task"[7] Quite recently the double-task method has received a very strong impulse from McDougall, who has designed a new apparatus for this special purpose. In his description of its possibilities he says: "in studying the concurrent or intercurrent performance of two different mental operations, the

[1] W. G. Smith: Mind, N. S. IV, 1895, 50.
[2] *Op. cit.*, 50.
[3] Drew: *Amer. Jour. Psychol.*, VII, 1895, 533.
[4] *Op. cit.*, 533.
[5] A. Binet: Rev. philos., XXIX, 1890, 138-155.
[6] Henri: L'année psychol., III, 1897, 232-278.
[7] Henri: *op. cit.*, VI, 1900, 250.

subject is first set to mark the spots while the cylinder rotates at such a rate that he easily puts his black mark on each red spot. The rate of rotation is then increased step by step until a maximal effort of attention alone enables the subject to mark each spot. He may then be set any other task that does not involve the use of his eyes or of his right hand, *e.g.*, the addition of figures dictated to him, the counting of irregular taps, the discrimination of two points touched on the skin, the performance with the fingers of the left hand of simple or compound reactions to auditory signals."[1] Unfortunately, McDougall himself has not as yet published any results obtained with this instrument, although his "Preliminary Communication" appeared in 1905. Finally, the double-task method reached the climax of its development in some of Wirth's experiments which we have discussed at length in a previous paper.[2] The observer had, for instance, to fixate a given point, to distribute his attention in various ways over the whole field of vision or over parts of it, and at the same time to expect and react upon a brightness change liable to appear in any part of the visual field. Thus the double-task method has, after all, been applied to a sufficiently large number of mental and physical activities to allow a fair comparison with the more frequent single-task method.

However, a true estimate of the relative value of the two methods based solely upon the results obtained with them is hardly possible, because in both cases the results depend very largely upon the effectiveness of the distraction material employed. Aside from this, the two methods are probably about equally well suited to serve the general purpose of varying the attention to a given task. If either, the single-task method has the advantage, because it is more like our everyday experience, where as a rule one set of mental processes occupies for some time the focus of consciousness, while others are forced into the background. Some authors, it is true, think that the power of distributing attention is a sign of greater intelligence,[3] but to take up this question would lead us too far from our problem.

We return, then, to a criticism of the effectiveness of the distraction-material. One reason for the ineffectiveness of a given task, whether attended to alone or together with another task,

[1] McDougall: Brit. Jour. Psychol., I, 1904, 438.

[2] Geissler: A Critique of Professor Wirth's Methods of Measurement of Attention. *Amer. Jour. Psychol.*, XX, 1909, 120-130.

[3] S. de Sanctis, *op. cit.*, 213; "dass das Vermögen, die Aufmerksamkeit zu verteilen, in der Psychologie eine höhere Bedeutung hat als das, sie zu fixieren."

is a lack of perfect continuity. It allows brief moments of interruption, during which the other task may rise to the focus, and thus be performed about as well as if it were done by itself. This holds especially in the case of sense-discriminations and reactions, which can be accomplished almost instantaneously. Another defect is that frequent repetition of the same task leads to habituation, that is, it involves less and less consciousness, and of course less and less attention; thus more attention can be given to the other task. Easy mental arithmetic and easy prose reading as well as many muscular activities are especially subject to this criticism. Finally, the complaint has sometimes been made that the distractions do not affect all normal subjects in exactly the same way. This is perhaps the main fault with odorous stimuli, although otherwise they appear to have more distracting power than any other source of distraction examined. Thus far it seems to have been impossible to find a task which is not open to one or other of these three criticisms. As Külpe says: "it is clear that, if we are to induce a determinate degree of consciousness, we must know, first, the mode of distribution of the total energy of attention over the separate conscious contents included within its range, and secondly the distractive value of the process which we have selected for our purpose. Now no general rule can be laid down under either heading; the results that have been obtained hold only in particular cases".[1]

The fact then that the means of varying the attention are themselves but little reliable is one of the reasons why several of the attempts at standardising the degrees of attention have thus far failed to give satisfactory results. However, there are many other reasons, which it will be best to discuss in connection with the various methods that have been proposed for, or employed in, the measurement of the attention. For the sake of systematic treatment we have divided all the methods into six groups. The first five may be classed together as methods of expression, analogous to the physiological methods employed in the investigation of the affective processes. According to these five methods the degrees of attention may express themselves in changes of 1, peripheral vision; 2, muscular strength; 3, liminal and differential sensitivity; 4, reaction-time; 5, accuracy of work. The sixth method may be likened to the method of impression. By a series of graded distractors different degrees of attention are to be induced in the observer, and he is afterwards to report which degree he experienced. We shall now discuss these methods in the order thus presented.

[1] Külpe: The Monist, XIII, 1903, 45.

I. The fact that *Peripheral Vision* is changed and narrowed under concentrated attention seems to have first been mentioned by Stanley Hall, in 1883. He says: "if we concentrate attention upon an image at the centre of the field of vision, its peripheral tracts seem to grow dark, as indeed does the centre itself with some observers, when the attention is fixed on a point in indirect vision"[1] Experimental evidence for this fact was submitted by Janet, who "concluded from it that the nature of the point fixated by the subject and the degree of attention she paid to it played a part in the extent of the visual field".[2] To quote him in full: "it is for this that we undertook a series of researches conducted as follows: At the centre of the apparatus, on the central point, we fasten a piece of paper on which, according to the case, some sentences or numbers are written very small. We place the subject in the proper position to measure her visual field; we close one of her eyes and request her not only to look at the centre but also to read the paper or mentally to make up a sum with the numbers written on the paper. When her attention is well fixed on the work, . . . we draw the stick on which the small white object is fastened over the perimeter from the external side of the eye that is being examined, moving from the periphery to the centre. We stop a few moments at the point which we know to be within the subject's field, 40°, for example. . . . We succeed thus in determining the subject's visual field during the fixedness of her attention. . . . With many normal men these conditions do not modify the visual field; with some they diminish it from 5° to even 10°; but with hystericals, and in general with patients whose attention is modified, this process brings with it surprising contraction".[3] Although Janet did not find a uniform contraction of the visual field with normal observers, he nevertheless thinks "that the phenomena of attention do not depend solely on movements (he probably has Ribot and perhaps also Münsterberg in mind), as has been said, for the modifications of the visual field cannot be connected with movements".[4] Instead, he would rather relate these contractions to "the power of personal perception", *i. e.*, the limited range of attention. "In short", he continues, "it would perhaps be possible to establish here the effort of attention in order to synthetize phenomena".[5] His facts were confirmed, as he himself points out, by Séglas, who had "established them independently, not aware of this first communication".[6]

[1] S. Hall: Mind, VIII, 1883, 180.
[2] P. Janet: The Mental State of Hystericals. Tr. by Corson, 1901, 71.
[3] *Op. cit.*, 71-73.
[4] *Op. cit.*, 73.
[5] *Op. cit.*, 73.
[6] *Op. cit.*, 71.

However, Wirth's recent experiments[1] do not seem to corroborate them. Wirth, it is true, did not investigate the extent of the visual field under various degrees of attention, but tried to establish changes in the difference limen in the peripheral regions of the field; nevertheless, it seems reasonable to expect a close relation between a higher limen and a narrower field. Unless, then, Janet's results are verified by other experiments repeating his conditions, there is no reason for assuming that degrees of attention can be expressed by the range of peripheral vision. Even if such a correlation could be established, it would still, although valuable in itself, be limited entirely to visual attention, and new methods would have to be found for other departments of attention.

II. *Muscular Strength* exerted on a dynamometer is supposed to express degrees of attention according to J. Loeb and his pupil J. C. Welch. They assumed, in the first place, that, "if P be the maximum pressure with simultaneous mental work, the more attention the mental work requires the larger $P\text{-}p$ becomes", and in the second place, that "if . . . P is the expression of the attention in the case that the whole available attention is given to muscular work, the expression $\frac{P\text{-}p}{p}$ represents the constant of attention for one kind of mental work".[2] In other words, "the maximum static innervation of the hand is arbitrarily selected here as a measure of other voluntary activities simultaneously attempted", so that "the determination of the constant of attention of various kinds of mental work affords a method of comparing the degree of concentration of attention required in different kinds of activity".[3] Thus the whole investigation depends upon the correctness of the assumption that "P is the expression of the attention". Loeb must certainly have known that static muscular pressure or "the maximum static innervation" is not simply a function of the attention, not even *mainly* a function of the attention. Does he, then, assume that the other factors involved, especially the physiological factors, remain constant and are, therefore, negligible? At least he fails to take them into consideration, while he makes no analysis of the whole situation which, from the nature of the task, must have been very complicated. His argument seems to rest upon the fallacy that attention is analogous or even equivalent to physical energy and may, therefore, be added to or subtracted from bodily strength and thus calculated in terms of kilogram-seconds. Another objection to Loeb's earlier work was raised by Mün-

[1] Wirth: Psychol. Stud., II, 1906, 30-88.
[2] Welch: Amer. Jour. of Physiol., I, 1898, 283.
[3] *Op. cit.*, 288.

sterberg[1] in 1888, but Welch makes no reply to it, as she systematically disregards the psychological aspect and literature of her problem.

III. *Sensitivity, Liminal and Differential,* has been proposed and used as a measure of attention by several psychologists. The original suggestion probably came from Fechner's work upon liminal intensities. The first experiments were made, upon Kraepelin's proposal, by A. Bertels in 1889 with the purpose of measuring not so much attention as distraction. Bertels used liminal brightnesses. However, his stimuli were not given simultaneously, but at determinate intervals of variable length, lasting from several seconds to fractions of a second. Besides, his assumption that "a stimulus, in order to reach a liminal value, must be the greater the less concentrated is the attention to it",[2] was not at all based upon experimental evidence, but simply taken for granted. Kraepelin himself did not seem satisfied with the results, for in 1896 he wrote: "the degree of concentration of the attention can be measured by determining the just noticeable stimulus intensity. However, such measurements, except where quite rough disturbances are employed, are so difficult to make and depend upon so many other presuppositions that, in spite of their great theoretical interest, for practical purposes they would hardly come into consideration".[3] In recent years the liminal intensity has been used by Wirth and Peters. The latter employed as stimuli (1) a liminal noise made by a shot falling through a variable distance upon a hard surface; (2) a liminal change in the illumination of a white cardboard produced by increasing or decreasing, in discontinuous steps, the resistance in an electric current feeding a light of 35 c. p.; and (3) a liminal pressure upon the nail of the middle finger. "The judgments of the observer were: noticed (+), perhaps noticed (+?), doubtful (?), not noticed (—). The observer had also to indicate after every experiment whether, in his subjective estimate, he had followed the instructions well, fairly well, or badly".[4] In some of the experiments, "the attention had to be concentrated as keenly as possible upon the expected impression", in order to "obtain normal liminal values". In another series the attention was concentrated upon a different activity, as reading, arithmetic, or dynamometer tensions. Finally, in a third test, the observer had to attend to whatever liminal stimulus might be given, without expecting any particular one.

[1] Münsterberg: Die Willenshandlung, 1888, 160 f.
[2] A. Bertels: Versuche über die Ablenkung der Aufmerksamkeit, Dorpat Dissertation, 1889, 9.
[3] Kraepelin: Psychol. Arb., I, 1896, 58.
[4] Peters: Arch. f. d. ges. Psych., VIII, 403.

Thus the *Einstellung* to any one of the three possible stimuli was practically eliminated. According to Peters these three series represent three different degrees of attention to the liminal stimulus, concentration with *Einstellung*, concentration without *Einstellung*, and distraction. He finds a uniform rise of the limen above the normal value in the experiments without *Einstellung*, while in the distraction series the results show great individual differences (*cf.* his Table 8, p. 416), though as far as the distractions were effective, there is a tendency of the limen to rise even higher than in the experiments without *Einstellung*. Of great interest to us is also the correlation of the results with the subjective estimate of the observer's attention to the distracting task. Peters, however, did not place great confidence in these statements, because he had found "long before that the criterion for the judgment of what is usually called attention is not (as, for instance, Münsterberg seems to believe) uniform at all, but is variable with different individuals, that one person judges the 'intensity' of his concentration by the strain sensations or by the intensity of the impulses of his will, another by the clearness and distinctness with which he has apprehended a conscious content, a third one finally by the affective components, his interest, etc."[1] Besides, Peters thinks that the estimates on different days cannot be compared. This would be true, of course, if one and the same observer varied on different days between the three possible ways of estimation; otherwise Peters' objection is not necessarily valid. If it were true, then the same criticism might be brought against judgments of intensity made on different days. Since Peters knew of the variety of means of estimating the attention, he might, if it had become necessary for his purpose, have overcome the difficulty by requiring his observers to use only one prescribed criterion. As it is, the most important addition which his investigation makes to the solution of our particular problem consists in the furnishing of experimental evidence for the fact that attention without *Einstellung* is not maximally intense.

IV. *Reaction-Time* was first used to express degrees of attention by Obersteiner. He assumed that the "retardation of the reaction . . . stands in inverse proportion to the intensity of attention".[2] The results show that with different people the same distractions have different effects. In this they agree with Wundt's and Buccola's results,[3] who also found that under distraction the time of reaction lengthened.

[1] *Op. cit.*, 428.
[2] Obersteiner: Brain I, 1879, 444.
[3] Buccola: Rivista di filos. scientif. I, 19.

While, on the one hand, Buccola, according to Stanley Hall,[1] thinks this to be "due to distraction or defective powers of voluntary attention", Wundt, on the other hand, is very cautious in the interpretation of the retardation. In the first edition of the *Grundz. d. physiol. Psychologie* he says, indeed: "as long as a disturbing secondary impression is present, so long it is impossible to raise the concentration of attention to its highest degree".[2] In later editions, however, this passage is omitted. Instead we read, for instance, in the third edition: "the innervation, which exists in every concentration of attention, probably varies with different sense-impressions, perhaps it arises in different localities of the centre of apperception".[3] Even this is entirely rewritten in the fifth edition, and the whole fact is simply referred to as "eine Verlängerung der Apperceptionsdauer des Reizes".[4] Nothing is said here about an intensified concentration of attention or apperception. Similarly careful in his interpretation of lengthened reaction under distraction is Swift, who found a small but uniform increase when the reaction was made while the attention was concentrated on some other task. He wrote: "I willingly admit that it is impossible to determine how far the instructions are observed in such a case".[5] He even believed that "there are no means by which the amount of attention given to the reaction or to the other task can be measured". Cattell,[6] who distinguished three introspective degrees of attention, concentrated, normal, and distracted attention, found that there was no corresponding lengthening of the reaction-time with less attention. Sharp, on the other hand, thinks it possible that "an individual who gave a quick reaction-time, owing to great power of attention, might, by having his attention artificially distracted, lengthen his reaction time until it corresponded to that of other individuals who normally gave long reaction times from small power of attention. Given a constant correspondence between length of reaction-time and degree of concentration of the attention, and the rapidity of reaction of an individual might be taken as an index of his power of attention".[7] Nevertheless she doubts the range of its practical application, while to us the basal fault seems to lie in the fact that "the constant correspondence" of which she speaks as given is not given, but is a very dubious matter,

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[1] Hall: Mind, VIII, 1883, 177.
[2] Wundt: *op. cit.*, 1st ed., 1874, 749.
[3] *Op cit.*, 3rd ed., II, 1887, 293.
[4] *Op. cit.*, 5th ed., III, 1903.
[5] Swift: *Amer. Jour. of Psychol.*, V, 1895, 17.
[6] Cattell: Mind, XI, 1886, 242.
[7] Sharp: *Amer. Jour. of Psychol.*, X, 1899, 16.

which has not yet been established by experimental methods. Binet and Henri also made use of the reaction experiment, not, however, for the sake of its retarded character, but for its irregularities as expressed by the mean variation. Quite recently Wirth and Kästner [1] have combined their perimetrical brightness-experiments with the reaction to the recognition of the brightness change, but their results are much too irregular to give any indication as to the different degrees of attention under which the task was done. Thus the final outcome of the reaction experiments, as used for the measurement of attention, has been on the whole negative; it has been impossible to establish a positive correlation between high degrees of attention and short reactions, and between lower degrees and correspondingly lengthened reactions.

V. The *Degree of Precision*, as the last method to be mentioned in this connection, seeks to find an expression of the degree of attention in the quality and quantity of work performed in a given time. At first sight this method seems to be the most natural and promising of all, but even here we find disagreement as to its efficiency. Some psychologists have assumed that, in general, changes in the quality or quantity of the work done are due directly to changes in the amount of the attention given to the task. Peters [2] calls this Bourdon's method. It has been recommended, for instance, by Jodl: "it is obvious that attention cannot be measured directly. However, its degrees may be determined indirectly by reference to its effects. These effects consist, in every task which does not have the character of an involuntary movement, in the avoidance of errors and mistakes and in the quickness of the execution".[3] Lipps, as we have pointed out, likens attention to a "psychische Kraft" which, like any kind of power, may be measured by the "Höhe der Leistungen".[4] In a similar way Stern holds that "the quality and quantity of a task performed under normal conditions could be used as a measure of the attentional energy".[5] This method has been used, for example, by Sharp: "the *degree* of attention habitually exercised by an individual was measured by the quickness and accuracy with which a certain given task was performed".[6] Hence her seven observers were classified accordingly, the fastest and most accurate as giving the best attention, the slowest and least accurate as giving the least attention to the same task. Vari-

[1] Wirth and Kästner: Psychol. Stud., IV, 1908, 139-200.
[2] Peters: *op. cit.*, 390.
[3] F. Jodl: Lehrbuch d. Psychologie II, 1903, 84-85.
[4] Th. Lipps: Leitfaden d. Psychologie, 1903, 34.
[5] W. Stern: Über Psychol. d. individ. Differenzen, 1900, 84.
[6] Sharp: *Amer. Jour. of Psychol.* X, 1899, 28.

ations of the attention of a single individual at different times could, of course, be detected and treated in a similar way. A modification of this method was first suggested by Friedrich in 1883. He said: "it is tempting to fix definitely the somewhat unsettled concept of attention by making it proportional to the measure of precision, *i. e.*, to the reciprocal value of the mean error, so that a small mean error corresponds to a high degree of attention, and conversely, a large mean error to a low attention, . . . since the characteristic phase of inattention consists rather in the large variations of the physiological times within the same experimental series". Although his results showed "a very good agreement between this theoretical consideration and experience", nevertheless he thought that "the identification of the attention with the measure of precision is in most cases open to objections. Only in cases of the simplest mental processes, which besides should be as homogeneous as possible and little subject to practice, may one assume that the mean error is mainly dependent upon the degree of attention. With more complex mental processes, however, not only does practice play a considerable part in the fluctuations of the individual experiments, but in certain cases the cause of great variations may lie in the mental processes themselves".[1] The experiments of Friedrich were made under Wundt's direction just before the publication of the second edition of the *Grundz. d. physiol. Psychologie*, where Wundt himself made the first change in his interpretation of the retarded reaction-time, so that it is probably safe to assume that Wundt agreed with Friedrich's contention. Nevertheless, the mean variation has been frequently proposed and employed as a measure of attention. H. Griffing, for example, says: "although we cannot assume that the average number of letters seen by an observer measures his powers of attention, the mean variation from the average of the numbers seen in the different experiments is presumably due, principally at least, to variations in the attention".[2] A wide application of the mean variation as a "dynamometer of the attention" is found in Oehrn's experiments. He gives,[3] for instance, the following *m. v.* values: for writing 2.6%, reading 3.4%, reading a foreign language (Latin, Livy) 3.3%, counting letters 4.2%, arithmetic 4.6%, counting letters in groups of three 5%, memorizing 12-place numerals 14.7%, and learning series of nonsense-syllables 27.4%, of the average value in the particular task. However, this work is criticised by Henri thus: "we do not

[1] M. Friedrich: Philos. Stud., I, 1883, 73 f.
[2] Griffing: *Amer. Jour. of Psychol.*, VII, 1895, 235.
[3] A. Oehrn: Psychol. Arb., I, 1896, 121 f.

believe that these figures express exactly the part due to attention in any one of the above cases, since other factors, like fatigue and practice, are not eliminated and certainly influence also the mean variation".[1] On the other hand, van Biervliet[2] maintained that "the attention can be measured by the consistency of the results, in fact, by the mean variation", and he applied this method in his experiments on visual acuity. Binet, reviewing and criticising this article, makes the following peculiar comment: "il y a dans l'article de M. Biervliet une remarque très ingenieuse, qui est a conserver. Il dit que l'attention se marque dans la variation moyenne d'une expérience; c'est tout a fait juste, dans beaucoup de cas. On le savait, mais personne jusqu'ici ne l'avait dit avec cette netteté".[3] Peters, whose work we have already discussed, found a close correlation between the magnitude of the *m. v.* in determinations of the liminal sensitivity and the degrees of attention under which he had established the limen. Nevertheless, even so he had to assume that practice had "a slight but distinct" [4] influence upon the results. Thus the mean variation can at best afford only a very rough measure of attention. Perhaps its greatest usefulness would consist in furnishing a convenient check upon other methods and results, especially since it is not bound up with any particular apparatus or procedure of experimenting, as is the case with all the other methods of expression.[5]

This review of experimental work brings to light the important fact that the expression methods have almost entirely neglected the factor of introspection. The main reason for this has probably been correctly stated by Peters; the investigators have not been able to agree upon a common characteristic feature of the state of attention. Some of them have emphasized the motor factor, some the affective components, some the strain sensations, but with the exception of Wirth none have used the clearness factor. This oversight is the more surprising as most of the theoretical discussions of the degree of

[1] Henri: L'année psychol., III, 1896, 244.
[2] Van Biervliet: Journal de psychol., I, 1904, 231.
[3] A. Binet: L'année psychol. XI, 1905, 71.
[4] Peters: *op. cit.*, 426.
[5] In addition to the five methods of expression thus far indicated we briefly mention the following method reported by Bliss, *op. cit.*, pp. 53 f. "During the course of my experiments Dr. Scripture suggested that the accuracy with which a person could steadily point to a given spot would be a measure of the amount of attention he could direct toward the work. In accordance with that suggestion the apparatus shown in Fig. 15 was arranged to measure this accuracy. . . . (The result was that) within certain limits the movements of the point increased or decreased inversely with the amount of attention given to the work".

attention have explicitly or implicitly referred to clearness and, as a matter of fact, have agreed upon it as one of the main characteristics of attention, while as to the other characteristics they heartily disagree.

VI. The only systematic attempt to employ introspection as an aid toward measuring the attention has been made by the method of *Graded Distractors*, as we may briefly call it, which is therefore the only representative of the impression-method in attention. By means of a series of stimuli which should make greater and greater impression upon the observer's mind, he would be induced to pay less and less attention to another simultaneous task. Stumpf, who considers it one of the two problems of a quantitative study of attention "to find means by which degrees of attention may be differentiated",[1] thinks that such means can be found, and gives as an example "the simultaneous perception of other simple phenomena which are capable of a progressive multiplication. The amount of this multiplication furnishes, of course, not a direct but an indirect measure of attention. Attention itself is not, but the means of varying it are, measurable".[2] Stumpf refers to Wundt and Boas as having already used this principle in their experimental work. Later it was applied by Drew in certain experiments. Their object was "to arrange a series of tasks of increasing degrees of complexity which should make ever greater demands on the mind, until the attention should pass from a fully concentrated to a completely distracted state".[3]

Although he accumulated bulky introspective records, Drew does not state in how far he succeeded with his series of distractions. After Drew, Stumpf's principle was made the leading problem of a series of minor studies in the Cornell laboratory, carried out during the years 1895-97. To quote Titchener: "Suppose, again, that we have arranged a series of distracting stimuli, homogeneous in kind but graded in complexity, such that we are able to reduce the observer's percentage of right cases from 100 to 95, 90, 85 . . . according to the distraction employed. It is necessary that the action of the distractors be constant; and it is necessary that they be of the same kind, and therefore exert an influence which differs only, and differs measurably, in degree. Having secured these conditions, we should let the observer decide whether the clearness of conscious contents was distinguishably different under a 5 per cent. and a 10 per cent. distraction, or under a 5 per cent. and a 15 per cent. distraction, or again under an 80 per cent. and an 85 per cent. distraction, and so on, all through the

[1] Stumpf: Tonpsychologie, I, 1883, 73.
[2] *Op. cit.*, 75. [3] Drew: *Amer. Jour. of Psychol.*, VII, 1895, 533.

series. We should thus finally obtain a scale of noticeably different clearnesses paralled by a scale of measured amounts of distracting stimulus".[1] However, the experiments have not yet led to the discovery of an entirely satisfactory series of graded distractors. It was found "that odors are admirably constant distracting material,—if only they could be measured"![2] Thus the impression method has had little more success than the expression methods, although psychologically it seems to be the sounder,[3] because it attempts to parallel experimental results with introspective data. The reason for its failure thus far is not inherent in its nature, but lies rather in the difficulty of finding satisfactory material, namely, effective distractions which are at the same time objective, homogeneous, and quantitatively measurable.

We have thus completed our review of the experimental work upon the problem of measuring the attention and may conclude, on the one hand, that, although much effort has been spent upon it, we are still far from a satisfactory solution. On the other hand, we are now better able to judge which paths would seem most promising for future experiments and which should be abandoned. We are also convinced that Pillsbury's suggestions on "*a priori* grounds"[4] are not at all hopeful. We miss in his chapter on "The Methods of Measuring Attention" the reference to the clearness factor, whose importance he emphasized in his introductory chapters.

PART II

An Attempt at a New Measurement of Attention in Terms of Clearness Values

The general aim of the following investigation was to renew the search for an exact method of measuring the attention by advancing simultaneously along old lines which the previous review of the literature had shown to be most promising, and along new paths which suggested themselves in the course of the experiments. Previous investigations had simply assumed that the variations of attention are directly paralleled by certain changes, whether in reaction-times, or in muscular pressure, or in quickness and accuracy of work, etc., etc., but they had not attempted to verify this assumption by reference to experimental introspection, and thus their psychological task was only half completed.

The purpose of our own work was to determine whether

[1] Titchener: Lectures on the El. Psychol. of Feel. and Att., 1908, 277.
[2] *Op. cit.*, 278.
[3] *Cf.* Ebbinghaus: Grundz. d. Psych., I, 1905, 621 f.
[4] Pillsbury: Attention, 1908, 92.

and in how far the assumed parallelism of the expression-methods rested on an experiential basis. This test might be applied in several different ways. For instance, one might argue that, if the assumption made by the expression-methods holds true, then it must be possible to find a direct parallel between any two ways of expressing degrees of attention, or rather, conversely: if two kinds of operations, which seem to depend mostly upon a strong concentration, give a positive correlation between different degrees of perfection, then the assumption holds true that their different degrees of perfection express different degrees of the attention with which they were executed. Or, one might argue thus: it would seem unreasonable to hold that objective variations in the work accomplished should indicate subjective variations in the observer's attention, if his careful introspection could not reveal such variations.

The former principle was applied in a first group of experiments, in which we compared the accuracy and quantity of work performed with the muscular rigidity or motor inhibition which is said to become the more pronounced the more attention is concentrated. Ebbinghaus, for example, has pointed this out in the following passage: "the same inhibitory relation which exists between purely mental states and processes, occurs also between them and bodily movements. In medium degrees of intensity both may quite well take place simultaneously. As soon, however, as on the one side the energy of activity is considerably increased, so soon will the other side suffer a decrease."[1] This fact had never been tested experimentally, at least for the purpose of measuring attention by it, unless one should include here Loeb's and Welch's dynamometer experiments. In order to get a graphic record of muscular rigidity the following arrangement was made. The observer's left arm was screened from his sight, and he was required to grasp with the hidden hand a handle fixed in an arm rest. The observer had no knowledge of the existence and purpose of the further details of the apparatus. The handle was in reality a plethysmograph, which was connected by glass and rubber tubing with a Marey tambour writing on a kymograph. The slightest movement of any of the five fingers, or any tightening or loosening of the grip, caused an appreciable excursion of the pointer, which increased with the extent of the movement.

The work which, after several trials with various materials, seemed best suited for an objective, quantitative gradation as to quickness and accuracy, was mental arithmetic in the form

[1] Ebbinghaus: Grundz. d. Psychol., I, 1905, 597.

of continuous adding. We arranged 4 different series of 30 figures each, selected so as to constitute 4 different degrees of difficulty. The easiest series contained all figures from 3 to 13, except 10, each figure occurring 3 times. The second degree of difficulty included the following figures: 23, 24, 25, 26, 27, 33-37, 43-47, and 53-57, each figure occurring twice. In the next series the following figures were used: 63-67 once, 73-77 and 83-87 twice, and 93-97 once. Finally the most difficult series contained: 128, 129, 131-135, 221-227, 314-319, 408, 409, 411-413, and 503-507. By using different starting points from day to day, practice or memory of the results was entirely eliminated, while the actual series did not need to be changed. The results are given in Table I. The following distractions were used: an electrically driven tuning fork of 512 vibrations, an electric bell, a metronome beating

TABLE I

Difficulty 1.	Difficulty 2.	Difficulty 3.	Difficulty 4.
To: 25, 36, 47, 58, or 64	To: 65, 66, 68, 72, 74, or 77	To: 29, 31, 34, 35, or 38	To: 18, 22, 29, 32, or 38
Add:	Add:	Add:	Add:
9	27	63	314
11	34	74	409
6	45	85	135
7	53	97	315
12	37	75	503
13	55	86	411
8	46	77	226
5	36	96	224
3	24	87	128
9	57	73	504
4	26	67	412
11	47	76	316
7	35	64	227
12	25	83	222
8	43	75	129
3	37	84	505
13	54	73	221
4	35	83	413
5	47	95	319
11	33	74	225
6	45	87	131
9	56	86	506
12	34	65	134
7	23	93	317
3	44	76	133
6	43	94	507
13	36	85	318
5	46	77	223
8	33	66	408
4	44	84	132

at 3 different rates, 60, 100, and 150 strokes per minute, and a flicker moving across the whole field of vision at 4 different rates of speed.

All experiments were done in a large dark room. Directly in front of the observer, at a distance of 2 m, stood a large screen, 1.20 x 2.40 m, covering practically the whole field of vision when the eyes fixated the central point. It was illuminated by an electric light above and somewhat behind the observer's chair. In front of this light a rotating spoke, made of a strip of cardboard, cast its flickering shadow over the visual field. A rectangular hole of 3 x 8 cm was cut in the centre of the screen, and through it the figures to be added were exposed. The figures of a series were pasted on a long strip of cardboard sliding in a groove directly behind the screen. To the upper end of the strip a string could be attached, which ran over a pulley and carried a counter weight. Another string, tied to the lower end of the strip, led to a spool with a handle, in front of the observer's seat. By turning the handle he wound the string around the spool, thus pulling down the strip and exposing the figures one after another at any desired speed.

As soon as the experimenter had announced the initial figure the observer began to add to it the figures exposed, calling out each individual sum until all 30 figures of a series had been added. The observer was instructed to concentrate all his attention upon the adding, and to neglect as far as possible any distraction, purposive or accidental. At the same time the experimenter behind the screen checked up each individual sum, marking every mistake, and also obtaining a graphic record of the time of each individual addition. This was done in the following way: one of the two ends of a pair of compasses carried a pencil with which the accuracy was recorded, while around the other end a copper wire was wound which was connected with an electromagnetic pointer writing on the kymograph. Next to the strip of paper upon which the accuracy of the additions was recorded, a long strip of sheet copper was fastened, connected by a wire with the other pole of the magnet. As soon as the observer pronounced a sum the experimenter touched the copper strip with the wired compass end; thus the electric circuit was closed, and the pointer raised by the magnet made a mark on the kymograph. A Jacquet clock furnished the time line by writing seconds. Thus for every experiment three graphic records were obtained; the movements of the observer's left hand, the time line, and the duration of each single addition as well as that of the whole experiment. The kymograph with the writing pointers was enclosed in a soundless box, a lever being so placed that the experimenter, by raising or lowering it with his left hand,

could easily and quickly start and stop the movement of the kymograph. After this was done, his left hand was free to manipulate any of the distractions to be employed during the experiment, while his right hand held the compass ready to check up the results.

After every experiment the observer was required to write out or to dictate answers to the following introspective questionary:

(1) What was your general affective mood during the experiment?
(2) Did it undergo any change, and if so of what kind and during which part of the experiment?
(3) Where and how intensive was your attention?
(4) Did it undergo any noticeable changes? If so, of what kind were they and when did they occur?
(5) Did you notice any internal sensations during the experiment? If so, of what kind were they and when did they occur?
(6) Did they distract you and to what extent?
(7) Did you notice any external stimuli? Of what kind were they?
(8) Can you remember in which order they occurred?
(9) Did they distract you and to what extent?
10) Can you add any other introspective observations not included in the previous questions?

It should be noted that questions 3 and 4 did not suggest any criterion for distinguishing different degrees of attention. The observers judged the intensity of their concentration of attention in terms chosen entirely by themselves. After some experience and practice in estimating their attention they developed, independently of one another and without suggestions from the experimenter, a scale of 4 or 5 different degrees of concentration. This fact was utilized in the second half of the experiments belonging to this group, by the adoption of the following arbitrary scale of degrees: very good or excellent, good, fair, bad, very bad; without, however, any definition of the nature of these degrees.

The experiments of the first group, including all preliminary and practice series, lasted from November, 1907, till March, 1908, when they were discontinued because they promised nothing new or of further significance. The observers in this as well as in the next group of experiments were Dr. W. H. Pyle (P), Dr. T. Nakashima (N), and Mr. R. A. Tsanoff (T). All three were graduate students in the psychological department, and had had considerable practice and experience in introspection. They observed regularly three times a week. Four experiments were made in the hour, and a rest of from 5 to 10 minutes was allowed between the second and the third, during which the observer could leave the dark room. In every hour, each one of the four degrees of difficulty occurred once. Their temporal order was so arranged that every difficulty occurred an equal number of times in the first,

second, third or fourth place. A long preliminary practice was required from the observer, and also from the experimenter. The addition tasks used in these experiments were slightly different in arrangement, but not in difficulty, from those employed in the later series. The regular series began in January, 1908, each observer making at first 24 experiments without any distractions. In the following 300 experiments, about 100 for each observer, the distractions occurred with the following frequency: the tuning fork, the electric bell, and each one of the four rates of flicker occurred three times, while each one of the three metronome rates occurred twice with every one of the four degrees of difficulty. The distractions were so distributed as to let each kind occur only once in an hour, and as often in the first place as in the other places. However, they did not persist during the whole experiment, but were used only during the first twenty additions, while the last ten additions were always performed in the normal way.

The results showed that, in spite of all precautions, a slow and gradual effect of practice and habituation made itself felt, especially during the first half of this group of experiments. The practice both reduced the length of time for the different degrees of difficulty and decreased the number of errors. The habituation manifested itself in a more and more pronounced disregard of the distractions, so that the first twenty additions were done as well as the last ten, and in some cases better, because the sudden interruption of the tone or flicker might confuse the observer for a few moments.

Considering the results as regards the degree of difficulty, we notice a uniform increase in the average length of time from the easiest to the most difficult series. Table II gives the results in seconds for the first 24 normal experiments, for all 96 experiments with distractions, and for the last 48 experiments with distractions. The item Limits shows the quickest and the slowest experiment that occurred with each degree of difficulty.

The overlapping between neighboring degrees of difficulty is due to practice, which brought it about that toward the end a more difficult task could be done as fast as an easier task had been done at the beginning. It is interesting to notice that, without exception, the more difficult additions reached the maximum of practice sooner than the easier additions. The shortest times for the former occurred after about six weeks of regular tri-weekly experimenting, while for the latter the shortest times occurred much later. The only difference of effectiveness between the different distractions was that the experiments with the flicker gave as a rule much shorter times and fewer errors than those with auditory distractions. Among

TABLE II

	Difficulty 1.		Difficulty 2.		Difficulty 3.		Difficulty 4.	
Obs.	Av.	Limits.	Av.	Limits.	Av.	Limits.	Av.	Limits.
P	90	75– 96	110	105–116	146	134–160	163	136–214
N	108	90–132	137	121–159	185	149–233	209	189–221
T	75	66– 84	117	100–135	154	136–160	180	166–201
P	73	57– 98	92	72–111	109	89–146	128	100–167
N	85	71–117	110	87–143	145	116–188	171	139–237
T	57	41– 96	103	81–131	116	84–145	152	114–190
P	71	62– 84	85	72– 94	100	89–115	116	105–137
N	80	69– 98	102	87–119	141	116–153	165	139–191
T	49	41– 61	100	81–128	103	84–123	138	114–173

the latter themselves no differentiation as to effectiveness could be observed.

In comparing the observers' estimates of attentive concentration with the calculated quickness and accuracy of their work, we shall refer only to the last 48 experiments, because here a uniform scale of degrees was used and the influence of practice was minimal as to improvement both in rate and in accuracy. In fact the number of errors was so small as to be entirely negligible. In order to discover whether there was any correlation between the observer's estimate of attention and the calculated time-values of his work, the latter were divided into 5 groups: quickest rate, fairly quick, average rate, fairly slow, and slowest rate. Because the number of experiments available from each observer was small, the results were combined for the purpose of calculation, after we had assured ourselves that the individual results were sufficiently similar to allow such a procedure. The 5 introspective degrees of attention were then paired with the 5 rates of work to give the following Correlation-Table:

TABLE III

Degrees of Attention

		Best	Good	Av.	Bad	Least	Total No.	Means
Rate of work	Quickest	24	1	—	—	—	25	1.04
	Quick	13	17	6	1	—	37	1.86
	Average	22	17	16	1	—	56	1.93
	Slow	8	6	3	4	—	21	2.14
	Slowest	—	4	1	—	—	5	2.20
	Total No.	67	45	26	6	0	144	
	Means	2.21	2.91	2.96	3.5	0		$r = .60$

The correlation coefficient was calculated according to the familiar Bravais-Pearson formula $r = \frac{\Sigma xy}{\sqrt{\Sigma x^2 \Sigma y^2}}$, which resulted in the equation $\frac{13.09}{24.29 \times 17.29} = .60$. This is a surprisingly high value, considering the fact that the observers were not told in what terms to define degrees of attention. They were, however, frequently warned not to judge these degrees on the basis of an estimation of the accuracy or quickness of the work, and as a rule they faithfully followed this instruction. The introspections do not, however, throw much light upon the methods of estimating attention used by the various observers. Occasional hints indicate a few general facts: in the case of P, the estimation was mostly based upon the ease with which the different sums occurred in consciousness. N usually relied upon his memory of the force with which the distraction impressed him; while T used as a basis the presence or absence of motor concomitants or "general nervousness". However, these criteria do not seem to have been used consistently or uniformly, nor were the observers always aware of their own methods. This fact tends to make the close correlation between the subjectively estimated degrees of attention and the objectively determined quantity of work the more remarkable, and should lead to greater confidence among psychologists in the observer's ability to make reliable estimates of quantitative aspects of his own consciousness. Our results are entirely different from those obtained by Peters, and our later experiments show that the trustworthiness of introspective estimation of degrees of attention can be still further increased by the adoption of a uniform principle of differentiating such degrees.

On the other hand, the plethysmographic curves, that were to have shown the muscular rigidity of the left hand, gave absolutely negative results. In most cases they were perfectly smooth, while with one observer they gave sometimes distinct pulse records. Out of 128 experiments with P, only 6 showed brief irregularities, with N this number was 17, with T 12; and most of them occurred in the very earliest experiments. Hence it was impossible to discover any kind of correlation between the state of bodily rigidity and mental work. In this respect, then, the application of our principle of comparing two expression-methods failed, mainly because of an unfortunate choice of the methods to be compared. The principle itself is not affected by these negative results, and awaits further test.

Since the results of the first group of experiments had incidentally shown how well even unsystematic introspection is

able to reveal subjective variations of attention, they offered a sufficient inducement for continuing these experiments as an explicit application of the second principle mentioned above. In other words, our problem shifted to this: How closely are the introspective estimates of attention correlated with quantitative changes of an objectively measurable character? In order to obtain systematic uniformity in the results, it was advisable to prescribe to the observer the terms in which he was to estimate the degrees of his attention. We followed Titchener's suggestion in asking the observers to base their subjective estimates of attention upon differences in the clearness of the mental processes attended to. In general, the procedure in this second group of experiments remained the same as in the first group. A few slight changes in method and material will be mentioned later, but the plethysmographic handle was not removed, chiefly because we wished to keep the external conditions of the experiments as constant as possible.

In order that the observers might become perfectly familiar with the introspective differences of clearness, they completed a large number of preliminary experiments, which were later repeated with those who observed in the third group of experiments. Besides, several undergraduate students, who at the time were members of the introductory laboratory course, were asked to take part in these preliminary experiments, mainly in order to see how easily they learned to distinguish between differences of clearness and differences of intensity.

The method in the preliminaries was in general this: two metronomes were set going at different rates, about 100 and 120 strokes per minute, and the observer, who sat with his back toward them, was asked to count the number of sounds between coincident strokes. Then the same stimuli were repeated, but the observer was required to perform some mental operation, for example, spelling or reciting in a foreign language, adding or subtracting successively the same number, naming rapidly the states with their capitals along the Atlantic coast, etc. Each experiment should not last longer than a minute or two. After several paired experiences of this kind, the observer is not only able to compare the differences between the clearness of the metronome strokes in the first and in the second case, but is also ready to assert that the strokes were not always equally clear or obscure. As soon as this stage is reached, the experiments are repeated with the same or similar material and the observers are encouraged to estimate, first the larger, later the finer, differences in clearness. Some observers will easily construct for themselves a scale of five or more steps from very clear to very obscure, while others will prefer to use rough differences in terms of per cent. Nearly all of our

observers, both experienced and inexperienced, found it relatively easy to apply some such gradation of clearnesses. In all these preliminaries the differences between the focal and the peripheral processes were rather large. The efforts to induce clearnesses which lay nearer the middle of the arbitrary scale were most successful when the observer was requested to hum to himself a more or less familiar melody and gently beat the time to it with hand or foot, while either one or both of the two metronomes were striking their own rhythms. Another effective method was to spell long phrases, or to add or subtract series of figures, to the rhythm of a metronome. After several days of practice the more experienced observers were able to give a fairly complete analysis of their consciousness during an experiment, and to estimate without difficulty the clearness or obscurity of every mental content experienced by them. It seemed, at least according to some of the observers, that the comparison of simultaneous processes as to their degree of clearness facilitated especially the task of analyzing a complete consciousness even in entirely different experiments.

After eight or ten hours of practice the observers P, N, and T had worked out for themselves, with suggestions as to a uniform terminology from the experimenter, the following scale of nine degrees of clearness:

1.	About	90-100% =	maximally clear	or excellent	attention
2.	"	80- 90% =	very clear	" very good	"
3.	"	70- 80% =	clear	" good	"
4.	"	60- 70% =	fairly clear	" fairly good	"
5.	"	50- 60% =	fair	" fair	"
6.	"	40- 50% =	fairly vague	" fairly poor	"
7.	"	30- 40% =	vague	" poor	"
8.	"	20- 30% =	very vague	" very poor	"
9.	"	0- 20% =	absolutely obscure	" no	"

Later, during the experiments in the dark room, all the observers added, of their own accord and independently of one another, an intermediate step between the first and the second degree, while T made one also between the second and the third degree.

The experiments of the second group were resumed in the dark room about the middle of April. The distractions which had been used in the first group were again employed, but now in various combinations, in order to find out whether this would make them more effective for inducing different degrees of attention. In the four simplest cases two auditory distractors were combined in the following way:

1. two metronomes of different rates, 100 and 120 per minute,

2. one metronome and the electric bell,
3. one metronome and the electric tuning fork,
4. the bell and the fork.

In the next four combinations the visual distraction caused by a medium rate of flicker was added to these pairs. Finally, three simultaneous distractors were given in this way:

1. two metronomes of different rates, the bell, and the fork,
2. two metronomes of different rates, the bell, and the flicker,
3. two metronomes of different rates, the fork, and the flicker.

The addition tasks were continued with one degree of difficulty only, but the figures 43-47, 53-57, 63-67, and 73-77 were given in the four different orders of Table IV. The number of single additions in one experiment was reduced from 30 to 20, so that the last ten (without any distractions) were omitted. Instead of this, during every hour one of the four experiments was completed normally, that is without any distractors, in order to be used as a standard of comparison. The normal series, as well as the series with the various combinations of distractions, were so distributed over the whole group of experiments as to occur with equal frequency in the

TABLE IV

To: 113, 115, or 117	To: 221, 225, or 229	To: 332, 335, or 338	To: 444, 445, or 446
Add:	Add:	Add:	Add:
56	47	46	73
45	63	64	55
67	56	73	64
74	44	57	47
54	73	43	74
73	55	66	54
63	76	55	65
44	66	77	56
55	77	65	75
77	46	54	46
64	65	47	63
46	54	74	45
53	45	63	53
65	67	53	67
76	53	48	76
57	75	75	57
47	64	67	77
43	74	56	44
75	57	45	43
66	43	76	66

first, second, third, or fourth place of an hour's work. After 3 days' preliminary work with normal series only, each ob-

server completed 60 experiments, of which 44 were done with distractions, so that each distractor combination occurred four times.

The questions for introspection were also somewhat modified, in accordance with the uniform standard of differentiating degrees of attention, and now ran as follows:

(1) Making the relative clearness of the adding process the criterion for degrees of attention, and calling the absolute maximal clearness which you can experience under most favorable conditions 100% or excellent attention, what percentage of attention or what degree of clearness prevailed, during the experiment, for the adding process?
(2) Was it constant, or intermittent, or changing to more or less?
(3) On the same basis of distinction, what degree of clearness or attention would you ascribe to other mental processes that occurred during the experiment?
(4) Were they uniform, or variable, and how?
(5) Were you distracted by anything else? If so, by what and how much?
(6) Was the distraction continuous or intermittent?
(7) What affective processes or moods dominated your consciousness during the experiment?
(8) Can you add any other appropriate remarks?

Besides, the observers were again expressly cautioned *not* to estimate their attention by the apparent temporal rate or the seeming correctness of their calculations. The general instruction was to concentrate attention upon the adding-process and to neglect as much as possible any intentional or accidental distractions. The large number of introspective data obtained from these, as well as from the other observers in later experiments, furnish interesting facts concerning the steadiness of attention and the problem of the two levels, which will be discussed later.

The results of this second group of experiments showed plainly that even the most complex combinations of distractors, after a few days' work, were unable to induce great variations of attention. Instead, toward the end of the whole group, most of the normal series were actually performed at a slower rate than the distraction series. Throughout the whole course of this group of experiments a slight and gradual practice effect was noticeable which, in the case of P and N, manifested itself in both a shortening of the time and in smaller deviations from the general average. In the case of T, however, a rather sudden improvement occurred about the middle of the whole course, while his variations from the average remained as great as in the beginning.

Although the distractions were not as effective as expected, yet the attention to the adding-process was not the same in all experiments, but varied between the first and the fifth degree; most frequently it was judged "very good", in a few cases it

sank to the fourth degree, once it was estimated by N at "fair", and once at "fairly poor"; but all these lower degrees occurred during the first few days, before the distractions had lost much of their effectiveness. In the case of P attention was most frequently judged at the highest degree, and only once or twice below "very good". This agrees perfectly with the quality of his work, the rate of which is much faster and the quality more uniform than that of the other two observers. Table V allows a detailed comparison of the subjective estimates of attention in terms of clearness with the average time (in seconds) of the experiments at these levels. With all three observers there is a perfect correlation between their best attention and their shortest time, and between correspondingly lesser degrees and longer times. The fractions given under the item Error have for their numerator the number of experiments belonging to each rubric, and for their denominator the number of errors occurring in these experiments. Wherever the m. v. is given as 0, only one experiment occurred under that rubric.

TABLE V

Degrees of Attention

O	Expts.	First Av.	MV	E	Betw. 1–2 Av.	MV	E	Second Av.	MV	E	Betw. 2–3 Av.	MV	E	Third Av.	MV	E	4 and 5 Av.	E
P	Total	54	4.2	$\frac{27}{21}$	57	3.7	$\frac{15}{15}$	63	5.5	$\frac{15}{15}$	66	0	$\frac{1}{1}$	—	—	—	—	—
N	No.	67	7.7	$\frac{4}{4}$	69	3.7	$\frac{18}{11}$	71	7.8	$\frac{32}{28}$	—	—	—	78	8.6	$\frac{7}{7}$	95	$\frac{4}{6}$
T	Expts.	—	—	—	60	9.6	$\frac{16}{20}$	64	9.5	$\frac{29}{23}$	79	9.5	$\frac{10}{9}$	87	5.0	$\frac{4}{3}$		
P	Normal	59	3.2	$\frac{8}{6}$	54	3.3	$\frac{4}{3}$	63	2.0	$\frac{8}{3}$	66	0	$\frac{1}{1}$	—	—	—	—	—
N		67	7.7	$\frac{4}{4}$	71	2.6	$\frac{8}{3}$	78	5.7	$\frac{8}{0}$	—	—	—	69	0	$\frac{1}{0}$	—	—
T	Expts.	—	—	—	63	13.	$\frac{6}{6}$	69	6.8	$\frac{7}{4}$	76	4.2	$\frac{4}{6}$	—	—	—	—	—
P	Distr'n	53	4.4	$\frac{19}{15}$	58	3.4	$\frac{11}{12}$	62	5.0	$\frac{12}{7}$	—	—	—	—	—	—	—	—
N		—	—	—	66	3.4	$\frac{5}{5}$	69	7.7	$\frac{29}{28}$	—	—	—	79	8.5	$\frac{6}{7}$	95	$\frac{4}{6}$
T	Expts.	—	—	—	55	9.4	$\frac{10}{14}$	64	9.5	$\frac{28}{19}$	83	11	$\frac{6}{3}$	87	5.0	$\frac{4}{3}$	—	—

In the whole Table there is but one case in which a lower degree of clearness shows a shorter average time than the next higher degree. This is due to the fact that all 4 experiments of the lower degree occurred within the last two weeks of work, while 6 of the 8 experiments in the higher degree came in the first three weeks, so that in this particular instance a cumulative practice effect obscures the correlation. On the whole, however, the shortening of the times due to practice was so gradual and constant that the correlation still holds, with but few exceptions in the case of N, if the whole period of experimenting is divided into 3 equal parts of 5 days' work each. We omit the table, because it is very like Table V. In a

similar way, we also fractionated the results with reference to the number of distractions used, and found that the correlation in this case holds with practically the same uniformity. The fact that the difference in the averages of two successive columns is only a few seconds is not surprising, if one considers that the difference in clearness itself is at best only very slight. On the other hand, the great regularity of these results and the general uniformity of the correlation, in spite of the very small differences in degrees of attention, are the best evidence for the general validity of our conclusion that the degrees of attention may be very accurately estimated by introspection, if they are distinguished in terms of the clearness of the mental process attended to.

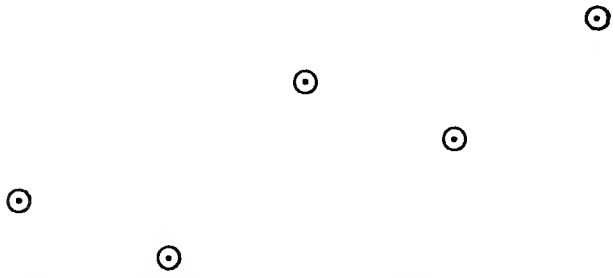
Because the experiments of the second group involved only the higher degrees of attention, and since they were not numerous enough for certain degrees, our next problem was to find a method that should induce all possible variations of attention to a given task. At the same time certain other requirements had to be fulfilled. In the first place, the efficiency of the work performed must depend as exclusively as possible upon the degree of attention given to it and as little as possible upon such factors as practice, fatigue, mood, etc. In the second place, the performance should never become entirely automatic or habitual, no matter how frequently it was repeated. Thirdly, it must require absolutely continuous attention, so that a momentary lapse should at once manifest itself in a momentary reduction of the quality of work. Lastly, the execution of the work must easily submit itself to a scale of quantitative gradations.

The kind of work which seemed to fulfill these conditions best of all was a modification of W. McDougall's method of marking irregularly arranged and step-fashion exposed colored dots or circles.[1] The exposure was made by means of the memory apparatus furnished by Spindler and Hoyer. The largest of the 5 aluminum cylinders belonging to it has a circumference of 48 cm, is 7.7 cm high, and gives 24 successive exposures. We fitted a large neutral-gray cardboard around the black metal exposure frame, in order to screen from the observer the whole apparatus and the experimenter. An electric light of 16 c.p. was fixed just above the exposure slit in the metal frame, so as to give a constant illumination and to do away with the shadow which by daylight the observer's hand and pencil cast upon the visual field. Another small screen protected the observer's eye from the direct light of the bulb. An

[1] McDougall: Brit. Journ. of Psychol., I, 1904, 438.

armrest was adjusted to the table in order to support the right arm from elbow to wrist, and to bring the hand exactly in front of the exposure slit.

The rings or circles to be marked were the common mathematical sign for the circle, approximately 3 mm in diameter (or 10-point of printers' size), as the sample shows. They



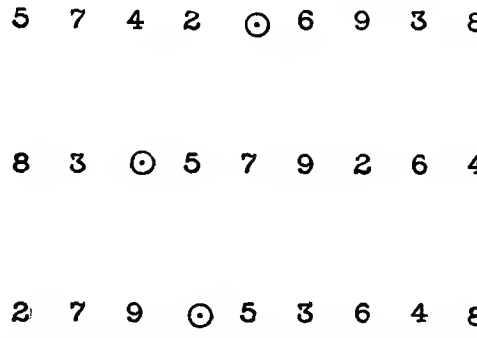
were printed in red ink upon strips of white paper which could be pasted tightly around the cylinder. The circles were arranged in 5 different irregular patterns which could be used in either direction so as to give really 10 different series. The lateral or horizontal deviations were also much larger than in McDougall's case, allowing for 5 different positions of the circles within a space of 32 mm, thus making the distance between two neighboring positions 8 mm. The greatest lateral deviation between two successive circles was 24 mm, or the length of three positions; however, this occurred only once or twice on each strip. The series thus far described will be referred to as "normal patterns", in distinction from the following "complication patterns" which were also employed. In one kind of series, which may be called the "letter pattern",

A M T D V ⊙ L U I

Y B L ⊙ A G P F Z

L S C U ⊙ V K H M

the circles appeared in rows of capital letters (10-point printers' size), each of the 24 rows on a strip containing 8 letters, their lateral distance from each other being 8 mm. The whole strip thus forms a sort of printers' pi, with the red circles mixed in. In another kind of series, the "digit pattern", the numerals 2 to 9 were substituted for the letters, so that each numeral occurred only once in a row. The figures show some



samples of each of the three patterns (with the exception that the circles are not painted in red). Finally another series was the "color pattern", in which little colored paper disks, of the size of the circles, were pasted on the strips of paper, in place of the letters or numerals. The colors used were blue, yellow, red, green, black, neutral gray, blue-green, purple, reddish orange, and yellowish orange. They occurred in irregular order, the last four also with irregular frequency, but, like the letters and numerals, about equally often in each of the nine possible lateral positions.

The marking of the red circles, which was done with a pointed, medium soft pencil, consisted in drawing a straight vertical line through the centre of the circle so that its length was about twice that of the diameter and extended the length of a radius above and below the periphery. Through the slit, 2 x 6.5 cm, in the black metal screen in front of the cylinder, one circle, or one whole row of letters, numerals or colors, with a circle among them, was exposed at a time for a fraction of a second, coming to a momentary rest and then disappearing quickly while the next row came into sight. The speed of the successive exposures could be regulated by means of adjustable fans, and it was so chosen for different observers as to be the fastest rate at which they could do the best possible work. The two observers who took part in the final series marked the 24 circles in 12 seconds; the three observers in the preliminaries,

who had less practice, varied between 15 and 22 seconds. The reason for drawing a line, instead of making a dot, as in McDougall's experiments, was that the former way of marking secured greater continuity of attention by filling out the entire interval of exposure, thus preventing momentary wanderings of attention, or at least revealing them by a bad mark.

In determining the quality of the task, the first four marks in each experiment were disregarded, in order to eliminate the effects of the initial imperfect motor preparedness. The 20 remaining marks were graded in the following way. On the basis of 100% for an absolutely perfect record, each individual perfect mark received 5%. A single error, which reduced its value to 4%, consisted either in a slight variation of the length or the direction of the line, or in the failure to pass through the centre. If two such errors occurred together, or if the line touched the periphery of the circle, the mark was graded 3%. If three errors came together, or if peripheral displacement was combined with a change in either length or direction, the value of the mark was 2%. Finally, if the line did not touch the circle at all, or if it could not be classified under any of the above headings its value was 1%, while an entirely omitted mark was graded zero. The sum of the 20 individual values thus represents an exact measure of the accuracy of marking during any given experiment.

For the purpose of inducing a greater range of degrees of attention to the marking test, our experimental arrangement offered numerous possibilities, because it involved three kinds of variable factors. In the first place, the stimulus itself could be either the normal pattern or one of the three combination patterns. In the second place, the task could be either a single task, namely marking the circles only, or a double task, namely marking the circles while performing simultaneously some mental operation like spelling, calculating, reciting, memorizing, etc. (other kinds are mentioned also by McDougall). Finally, the direction of the attention was variable; it could be shifted from the marking to the mental operation or even to the accompanying letters, numerals or colors.

In the autumn of 1908 a number of preliminary experiments were done, mostly for the sake of a general orientation. The observers were Prof. I. M. Bentley (B), Dr. W. H. Pyle (P), and Mr. T. Okabe (O), the latter a graduate student in the department of psychology, with considerable experience in introspection. B and O took part in 30 and 40 experiments respectively, while P finished as many as 60. These preliminaries showed that the method was adequate to our purpose and that it also fulfilled the other requirements. Hence the experiments were continued in February and March, 1909, with

Miss H.-M. Clarke (C) and Miss A. de Vries (V) as observers. Both were pursuing graduate work in psychology and had several years' training in experimental work. They each completed 130 of the final experiments, in which the following combinations of the three variables occurred:

	Stimulus	Task	Direction of Attention
1.	Normal pattern	Marking circles only	To the Marking task only
2.	Letter pattern	" "	" "
3.	Digit pattern	" "	" "
4.	Color pattern	" "	" "
		and	
5.	Normal pattern	" Spelling	" Spelling
6.	Letter pattern	" "	" "
7.	Digit pattern	" "	" "
8.	Color pattern	" "	" "
		and	
9.	Normal pattern	" Reciting	" Reciting
10.	Letter pattern	" "	" "
11.	Digit pattern	" "	" "
12.	Color pattern	" "	" "
13.	Letter pattern	Marking circles only	To the Letters only
14.	Digit pattern	" "	" Digits "
15.	Color pattern	" "	" Colors "

Other combinations had also been tried in the preliminaries, but with less success; for example, interpreting as letters certain tactual impressions upon the left hand. In the combinations 13 to 15 the direction of the attention to the letters, digits, or colors, was aided by requiring the observer to form a judgment, say about the most frequent or least frequent letter, numeral, or color, or about some combination of them. In the case of the combinations 5 to 12 the marking of the circle was done on a lower level of consciousness; it had, so to speak, to take care of itself.

From 5 to 8 experiments could be finished in one hour. At the end of each experiment the following questionary was answered:

1. How much attention (in terms of clearness) was given:
   (a) to the marking of the circles?
   (b) to the other required task?
   (c) to other sensory or ideational processes?
2. How steady was this clearness (or: within which limits did it vary), and which degree of clearness was the most frequent with regard to:
   (a) the marking of the circles?
   (b) the other required task?
   (c) the other mental processes?
3. What affective mood prevailed during the experiment?
4. At how much is the quality of marking the circles estimated in terms of per cent.?
5. What other relevant comments can you make upon this experiment?

There was at first some difficulty in the estimation of the

clearness of the marking. This task involves the visual perception of the circles and their positions as well as kinæsthetic sensations from the quick motor adjustments of the hand and the movements of the pencil. After some practice, however, these factors fused into a conscious complex of homogeneous clearness which was easily estimated. Rare exceptions to this were always revealed in the introspections, and if in such cases the differences in clearness between the three factors were only slight, their average was accepted as a final estimate, while if the differences were more than two degrees of clearness, the experiment was disregarded and repeated later on. In a few cases, observer V felt unable to make a judgment about the clearness of the marking, because her attention was too intensely directed to some other task. These doubtful cases were classed in the ninth degree, which represents "least or no attention."

The results of the marking tests agree perfectly with those of the adding tests, and extend the correlation between degrees of attention and quality of work to the lower levels of clearness. Table VI shows the average value and the mean varia-

TABLE VI

	Degrees of Attention							
	First	Second	Third	Fourth	Fifth	Sixth	Seventh	8th and 9th
Obs.	Av. MV	Av. MV	Av. MV	Av. MV	Av. MV	Av. MV	Av. MV	Av. MV
C	76 4.9	71 3.0	67 5.3	61 4.5	55 5.0	49 6.0	40 6.9	29 7.6
V	77 3.4	70 3.5	63 5.3	51 5.3	47 5.0	43 4.0	41 5.6	40 5.6
P	85 2.1	77±2.9		71±6.3		55±8.5		34 11.3

tion of the experiments made at the various clearness levels. Only in the case of P the values obtained at two levels were combined, because they were not very numerous, while the results of B and O, although they show a similar tendency to correlation, were too few to justify their tabulation. The results of C and V were treated also by the Bravais-Pearson formula. For this purpose, the whole range of the calculated values of work, from the lowest to the highest per cent. was divided into nine equal parts, which were then compared with the nine degrees of clearness. The correlation-coefficients obtained under this arrangement were .83 and .73 from C and V respectively. These two figures show beyond doubt that introspectively distinguishable variations of attention are very closely paralleled by corresponding differences in the accuracy of work performed at these different levels of attention, provided that the estimation of degrees of attention is done in terms of clearness values, and that the work itself is as little

as possible influenced by anything else but changes in attention.

The objection might be raised that our observers based their judgment of degrees of attention upon an implicit estimation of the value of their work; this estimation is easier, since daily life offers many opportunities for practice in judging one's own work and ability. If this were the case, however, the correlation between the estimated and the calculated value of the work should at least be just as great as, if not greater than, the correlation between estimated attention and calculated work. But the correlation coefficients for estimated and calculated work were .76 and .28 for C and V respectively, as compared with .83 and .73 in the previous correlation. This difference shows plainly that our observers used a criterion for the judgment of degrees of attention different from that employed in the estimation of the quality of the work. What their criteria were can be best shown by quoting from a summary introspective description given by C and V at the close of the entire work with the marking tests.

Observer C who, like V, preferred to estimate clearness differences in terms of per cent., employs "a sort of schematic visual image of 100, rather complex," but somewhat different from her customary visual image of the whole number-scale. She described the latter as a line going out to the right in a horizontal plane, while the visual clearness scale is a line that goes out straight in front of her, with its farther end slightly raised. She goes on to say:

"Clear out at the other end I can see visually the 100, and that is the clearest part for me of the whole thing, which is becoming very shadowy toward me. The clearness degrees represent themselves to me by the approach they make to the 100 mark. It is a matter of clearness in memory; if the circles stand out plainly in memory (after the experiment) I put them high on the scale, if not, I put them low. There seems to be a cumulative effect of the twenty-four circles in any experiment. Sometimes, however, a single circle stands out strongly, but this does not affect the whole estimate. . . . I find a great difficulty in noticing strain, because something else is so much more interesting that the strains are too much in the background. Sometimes the strains are noticed later (after the experiment), as *e. g.* in a fatigued hand, but ordinarily they are too much in the background to be remembered even later." As to her judgment of the quality of work, C says: "the ease with which I mark enters sometimes into my estimates. I also had often some kind of a general impression as to how many good and bad marks there were. If I could not remember this I took it for granted that my work was poor. It was always pleasant when the work seemed easy, and unpleasant when it seemed hard. The cumulative effect of these individual feelings influenced my judgment of the quality of the work."

These introspections are in close agreement with her re-

sults. She has fairly definite, reliable, and constant criteria both for her clearness estimates and for her judgments of work, and hence her correlation coefficients are remarkably high, namely .83 and .76 respectively. She was never in serious doubt about either of her two estimates, and hesitated only rarely in cases of the very lowest degrees. In this respect she differs widely from observer V, who in 20 out of the 130 experiments was unable to tell what kind of work she had done, especially if her attention had not been directed to the marking. V was also much more uncertain about the lowest degrees of clearness, and the averages of work (given in Table VI) for these lower levels are very little different from one another. Her final introspections indicate that she was aware of this weakness, and they also show plainly that her clearness estimate could not have been influenced by an evaluation of her work. She says:

"I valued the circles in terms of clearness. Sometimes I could distinguish between their visual clearness and the motor clearness of marking. Sometimes I was not conscious of seeing the circles at all. The thing attended to was always very much clearer than the others, which receded the more into the background, the higher the other process was. My standard is either maximal or minimal, that is, I estimate from "no attention" upward, or from "maximal attention" downward. With my strongest attention there is no strain sensation, but after the experiment is over I feel relaxed, from which I infer that I must have been strained before, although I did not notice it, because my attention was too good on the task. The scale was not visualized; it is simply a matter of clearness of the object attended to. The quality of work had never influenced the estimate of the attention. In fact the estimate of the work was mostly a guess. I never remember the marking of the circles collectively. If I saw myself making a bad mark I judged my work usually very poor. When my attention is on some other task, I certainly do not focus my eyes upon the circles, and the marking seems to do itself. It must be analogous to the automatic writing of answers to questions without hearing the questions."

There can be no doubt, therefore, that the objection mentioned above does not hold.

The main problem of our investigation has thus received a satisfactory solution. We have shown that the assumed parallelism of the expression-methods, especially as far as the measure of precision is concerned, rests upon solid introspective evidence, if clearness is accepted as the essential characteristic of attention. At the same time we believe that our results have brought us within reach of a new and fairly definite method of measuring attention, for the results have shown that degrees of clearness are just as accessible to introspective determination as variations of the intensity of sensations. This was somewhat surprising to ourselves, because we doubted in the beginning the adequacy of introspection, and

expected at best only a rough parallelism between estimated degrees of attention and calculated degrees of precision in work. There is sufficient warrant in our results for the assumption that continued practice will lead at least to a differential clearness limen which may be just as definite and precise as any other psychophysical difference limen, while on the other hand it may be perhaps more difficult to establish the least possible or the highest possible clearness degree for any given mental process. However, the determination of a difference limen for clearness would be the most important step toward an exact measurement of the concentration of attention. It would enable one, by starting with a certain clearness degree of a given mental process under fixed experimental conditions, either to increase or decrease that clearness by just noticeable differences, until the maximum or the minimum of attention to the particular process is reached.

The results of our experiments also give some indication that the increase or decrease in clearness depends not so much upon the nature and number of distracting stimuli as upon certain other factors. The chief among these seem to be the kind and complexity of the tasks involved and the instructions or directions for subsequent introspection. Peters had already shown that attention with *Einstellung* is more concentrated than without it. Now the *Einstellung* itself depends largely upon the *Aufgabe* or instruction. If only one task is to be performed, as, for instance, in our case, the marking of the circles in the normal and the combination patterns, it is necessary before every experiment to state at least whether maximal or normal attention is required, otherwise the *Einstellung* is determined by chance and will result in a larger range of degrees of attention. If two tasks are to be performed simultaneously, for example, the marking together with the reciting, the spelling, or the counting of certain combinations, then it must be indicated which task is to be attended to, and to what extent the other may be neglected. Such an instruction is easier to follow than the requirement to give to both tasks an equal amount of attention, especially if they are very different. How far the nature and complexity of the tasks to be performed determine the degree of attention given to them is shown in Table VII. The vertical columns of figures headed by the 9 degrees of attention indicate the frequency with which the marking in different experiments was done at the various levels. The individual means for each observer as well as the combined means are shown in the last two columns, while the column before gives the total number of experiments in each case. In the first four rows of the Table the clearness of the marking test when done alone is compared with the clearness

TABLE VII

No.	Nature of Expts.	Obs.	Degrees of Attention 1	2	3 4	5 6	7 8 9	T.	Means Ind.	Comb.
1	Norm. and Comb'n-P	C	9	20	9 5	1		44	2.32	2.3
	Marking Circles	V	13	13	10 7	1		44	2.31	
2	Norm. and Comb'n-P	C		3	2 8	14 1		28	4.28	5.0
	Marking and Reciting	V			1 4	6 7	8 2	28	5.81	
3	Norm. and Comb'n-P	C			2 4	9 7	4 2	28	5.46	5.8
	Marking and Spelling	V			3	5 8	7 5	28	6.21	
4	Combination-P	C			3 2	6 6	7 3 3	30	6.1	6.2
	Attention to Comb'n	V			3	4 8	11 2 2	30	6.3	
5	Normal Pattern	C	4	9	9 8	6 3	1	40	3.4	3.54
	All possible Tasks	V	10	8	3 4	5 3	3 4	40	3.67	
6	Letter-Pattern	C	1	6	3 3	9 2	6	30	4.43	4.9
	All possible Tasks	V	1	2	4 3	3 6	8 2 1	30	5.37	
7	Digit-Pattern	C	2	5	1 4	9 4	1 2 1	29	4.55	4.8
	All possible Tasks	V	2	2	4 2	3 8	7 1	29	5.0	
8	Color-Pattern	C	2	3	3 3	5 6	3 3 2	30	5.0	5.3
	All possible Tasks	V		1	3 5	5 6	8 2 1	30	5.58	

of the same test when most of the attention is given to reciting, to spelling, or to some combination of letters, digits, or colors. The latter kinds of task require very high degrees of clearness for themselves and permit only very little attention to be given to the marking, namely about the 6th or 7th degree as a rule, the two means being 6.1 and 6.3 for C and V respectively. Spelling allows more attention to be given to the marking, and reciting still more, while of course the marking alone may involve the very highest degree of clearness. The influence of the complexity of the patterns is shown in the last four divisions of the Table. Work done with the normal pattern permits as a rule the highest attention to the marking, the letter and the digit patterns allow very much less, while the color pattern allows the least attention to the marking of the four kinds of patterns. However, we do not wish to place too much emphasis upon these results, because the number of experiments is not large enough for general conclusions of this sort to be based confidently upon them.

Finally, another point in regard to which the large introspective material accumulated in the course of our investigation reveals some interesting evidence, is the number of simultaneous clearness levels and their relative distance from each

other. It is frequently maintained that extreme concentration to one group of mental processes is accompanied by an equally extreme obscurity of other mental processes forming a general background, and that less concentration is simultaneous with correspondingly less obscure background processes. Pillsbury expresses this by saying: "the amount of attention is practically constant and cannot be applied to one object without affecting the clearness of others".[1] So far as our knowledge goes, such statements have been based on casual observations only, and we have, therefore, thought it advisable to present the following Table VIII as giving a numerical expression to the facts in question. The 9 large horizontal divisions indicate the 9 clearness degrees which by introspective estimation were assigned to the marking test. The figures in the third vertical column tell how frequently with each one of the three observers O, P, and V, the marking was estimated at any particular level. The next 9 vertical columns of figures show in how many cases other mental processes simultaneous with the marking at a certain level were mentioned under a certain degree of attention. Thus, for instance, in 24 experiments O estimated his marking at the highest level and in the same experiments assigned to other simultaneous processes in 2 cases the 6th degree of clearness, in 10 the 7th degree, in 12 the 8th degree, and in 6 cases the lowest or 9th degree. In some of these cases an observer was able to mention 2 or 3 individual contents and to assign to them either the same or slightly different degrees, in other cases he would give merely their average degree, while in a few cases he remained doubtful. The last mentioned were omitted from the Table, likewise those experiments in which the attention to the marking varied between more than two neighboring degrees. In the case of observers O and P several practice series were included in order to get a sufficient number of results for tabulation. The Table shows as definitely as can be expected under our experimental conditions that the frequency-modes of distribution for the simultaneous processes at first gradually approach, starting with 1 and 7, then 2 and 6, then 3 and 5, then they fuse in the 4th and 5th horizontal division, and finally they change places with each other, occurring now at 6 and 3, at 7 and 1, at 8 and 1, and lastly at 9 and 1. At the 4th and 5th degree of marking, there is no mode distinguishable for the other mental processes, because here we have a more or less uniformly distributed attention without much of a focus or a background. At the 6th level, however, the dual division becomes again apparent and remains so for the rest of the Table. This

[1] Pillsbury: Attention, 1908, 9.

TABLE VIII

No.	Obs.	T.	Degrees of attention 1	2	3	4	5	6	7	8	9	Total
	O	24						2	10	12	6	30
1	P	13		2	1		1	5	6	2	4	21
	V	33							4	1	5	10
	3 O's	70	0+70	2	1		1	7	*20*	15	15	71
	O	8				1	1	5	1			8
2	P	22				2	3	9	8	5	1	28
	V	13				1	4	1	2		3	11
	3 O's	43		0+43		4	8	*15*	11	5	4	47
	O	0										0
3	P	13				2	7	6	4	2	1	22
	V	14		1	2	4	1				7	15
	3 O's	27		1	2+27	6	*8*	6	4	2	8	37
	O	1		1								1
4	P	5			1	1	3	2	1			8
	V	14	1	2	7	3	1	1			3	16
	3 O's	20	1	3	*8*	4+20	4	3	1		3	25
	O	3	1	3				1				5
5	P	5			4		1		3	1		9
	V	16	5	4	3	3	1	1			5	22
	3 O's	24	6	7	*7*	3	2+24	2	3	1	5	36
	O	6		4	1				2		2	9
6	P	6			3	1	1	2	3	2		12
	V	23	6	5	7	1	2	2			8	31
	3 O's	35	6	9	*11*	2	3	4+35	5	2	10	52
	O	6	2	2	2	1			2	3	1	13
7	P	0										0
	V	26	19	2	4	3	2	1			12	42
	3 O's	32	*21*	4	6	4	2	1	2+32	3	13	55
	O	8	3	6						3	3	15
8	P	3		2	2				1	2		7
	V	6	6								2	8
	3 O's	17	*9*	8	2				1	5+17	5	30
	O	3		3	1	1						5
9	P	0										0
	V	5	4	1		1					6	12
	3 O's	8	4	4	1	2					6+8	17

mutual relation of the two clearness levels in an attentive consciousness may perhaps be compared to the two levels of a quicksilver column occupying about half the space of a U-shaped glass tube. The one end of the column cannot rise to its maximal height without depressing the other to a minimal height, nor can the one end move toward a medium high point

without causing the other end to approach approximately the same level, but of course from the opposite direction. This analogy can apply, however, only to the two-level formation of attentive consciousness, which since Leibniz has been known under the terms apperception and perception.

This distinction has not been unanimously accepted by modern psychologists as a universally valid description for all possible states of attention. Some of the authors[1] discover in their own consciousness processes of more than two levels of clearness, while Wirth even seems to hold that at any moment of attention all possible degrees of apperception may be represented by simultaneous processes and that only "under certain conditions, which favor a kind of dual division, it is possible that a region, to which a fairly uniform attention is given, may be opposed to a 'background' or 'periphery' of consciousness, to which attention is as uniformly denied".[2] To this statement we may reply that according to our results the occurrence of the two-level formation in the attentive consciousness does not depend upon "certain conditions", but upon the individual observer. We believe we have sufficient evidence to justify the assumption that there are two more or less distinct types of observers, those for whom the two-level division is the most natural and most common, and those who experience as a rule several levels of clearness.

Of our 5 observers B, C, O, P, and V, who worked under identical experimental conditions, the last three reported without exception the dual division of clear focus and vague background. There is, of course, the possibility that during the experiment, which lasted at least 12 seconds, processes of an intermediate degree occurred but were too fleeting to be recalled later. The first two observers, B and C, on the other hand, frequently mentioned such processes on intermediate levels of clearness. Here again it might be objected that our experiments did not deal with a simultaneous consciousness, but with successive states of attention, which might have exhibited a variable maximum of clearness and thus, if viewed as a whole, would present simultaneously what really was experienced in succession. We can only offer in reply the introspective material of our observers. We therefore quote a few passages from their introspections:

Observer B

(1) Clearness of digits varied from 3-7, some attention was given to the whole row, a few individual digits were as high as 3 in clearness. Circles pretty dark, never higher than 5, and usually 8, while the

[1] *Cf.* Titchener's discussion of this topic, Lectures, 220-228.
[2] Wirth: Phil. Stud. XX, 493; *cf. Amer. Jour. of Psychol.* XX, I.

marking was almost automatic, about 7-8. Other processes, *e. g.*, the noise of apparatus darkest, 8-9.

(2) Clearness of colors as whole rows 4-5, marking visually 5, as motor adjustments perhaps 6. In the background mostly verbal comments, 6-7. Sometimes flashes of colors, especially red and yellow, rose occasionally as high as 3.

(3) Focal clearness was distributed among the circles which got 3. Letters were quite varying, those next to the circles between 3 and 6, others 8. The general conscious background was very dark and dead, about 8 or 9, mostly verbal images, saying "I must avoid the letters", or noise of apparatus, and the general experimental setting.

(4) Focus on average 3, mostly visual and kinæsthetic. The background as a rule 8, made up as usual. A few distractions, chiefly verbal ideas, rose as high as 5 at times.

Observer C

(1) Marking of circles 90%. Individual colors not noticed, perhaps 5%. But the rows as such were confusing, the difficulty of finding circles about 50%, being mostly strain in hand and eye accompanied by verbal comment.

(2) Marking the circles about 80%, the digits not more than 10%, they were scarcely seen. But the noise of the apparatus sometimes rose as high as 40% and was markedly unpleasant.

(3) Marking 70%, colors about 30%, noise of apparatus 50%, rose so high especially when it suggested ideas of railroad, which were mostly visual and about the same in clearness.

It must be added that even with these two observers the dual division was experienced at times; but it is significant that B, previous to his participation in our experiments, had firmly believed in the universality of the two-level formation. Among the undergraduate students who took part in the preliminary experiments on clearness, we also found some insisting that in their experiences during these experiments as well as in their daily life the dual division was only rarely realized. We must, of course, admit that in daily life we do not usually analyze our consciousness, nor do we then sharply distinguish between psychological clearness or prominence in consciousness, logical clearness of the meanings and relations, visual clearness or distinctness, and differences in intensity of simultaneous processes. Nevertheless our observers were aware of these distinctions at least during the hours of experimentation. Adding to this the fact of the difference of opinion among expert psychologists upon this very point, we are led to make the assumption of two types of attentive consciousnesses, the dual division type and the multi-level type.[1] Whether these two types are mainly a matter of individual difference of constitution or of training and habit, or whether they depend upon certain psychophysical conditions within the same individual, must be left for experiments to decide. The distinction is not

[1] Such a possibility is, if not admitted, at least implied in Titchener's Lectures, 228.

intended to be a substitute for Titchener's hypothesis that in the two-level formation there may occur slight differences of clearness both in the focus and in the periphery, but is offered rather as a supplement to serve its purpose until a better explanation is proposed to account for the differences of introspection regarding the number of simultaneous clearness levels in the attentive consciousness. Our results furnish, as a matter of fact, many instances for the coexistence of small clearness differences on both the upper and the lower level of attention.

In conclusion we may briefly restate the general results of our investigation :

(1) A very close parallelism was found to exist between introspectively distinguishable variations of attention and corresponding differences in the precision of work performed at these levels, under the condition that the estimation of degrees of attention was made in terms of clearness and that the work itself was not influenced by anything else but change in attention.

(2) Under the same conditions the introspective estimation of the quality of the work was not as reliable as the evaluation of the degrees of attention.

(3) It seems possible that by continued practice a differential clearness limen may be established which would be of great assistance in the measurement of attention.

(4) The degree of concentration does not depend so much upon the nature and number of distractions, as upon the nature and complexity of two simultaneous tasks and the preliminary instructions regulating *Einstellung* and direction of attention.

(5) There seem to be two types of the attentive consciousness, the dual division and the multi-level formation.

(6) In the dual division type of attention a reciprocal relation exists between the two levels; that is, the higher the apperceptive level rises, the lower the perceptive level falls, and conversely.

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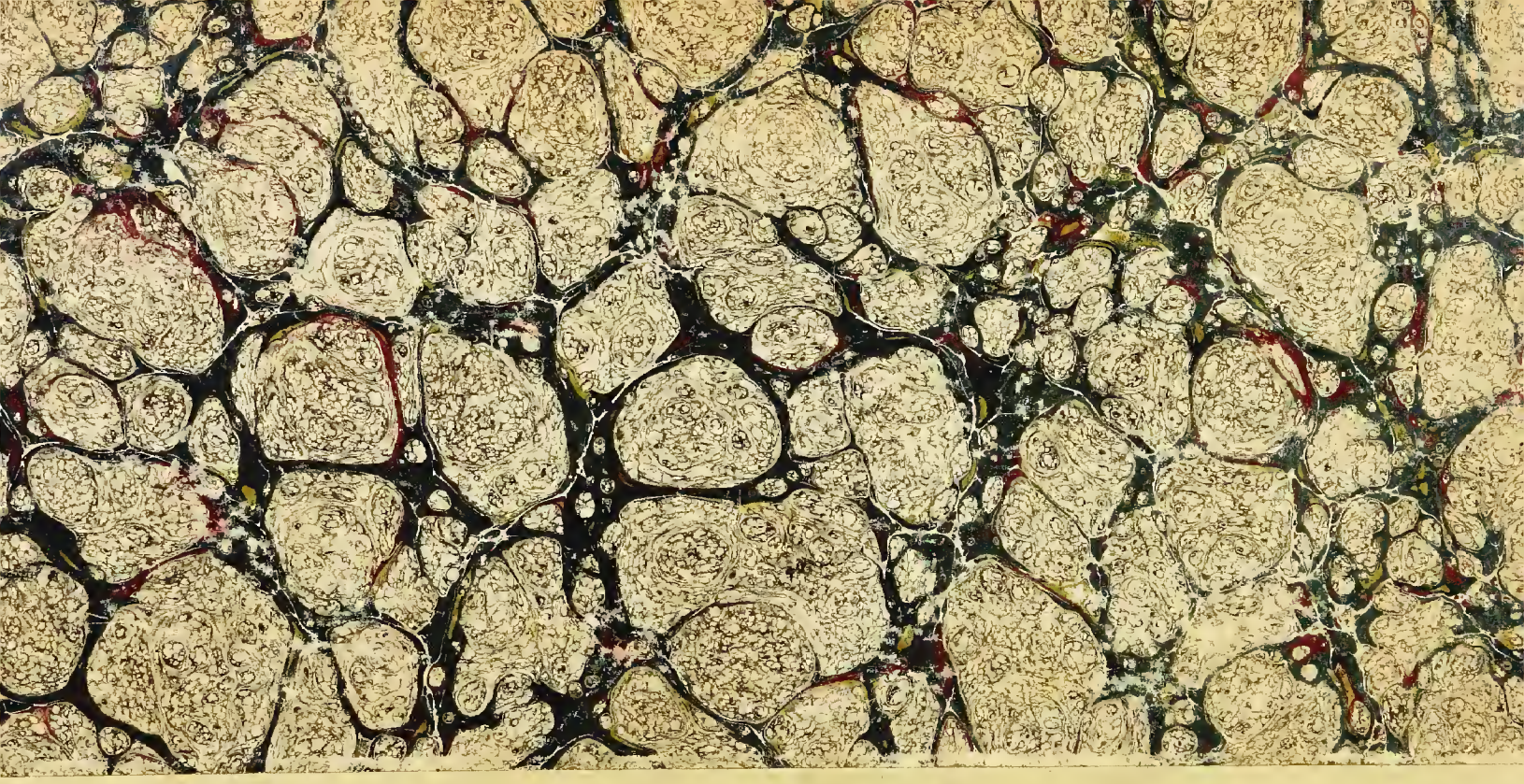
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